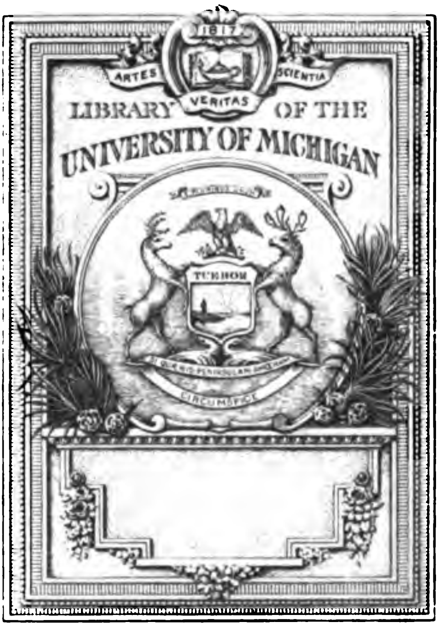
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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 15, 1883, *to April* 24, 1884.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 15, 1883.

Professor T. H. HUXLEY, President, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

The Rev. Percival Frost and Mr. Howard Grubb were admitted into the Society.

General Boileau, Professor Crofton, Mr. Hind, Dr. W. Pole, and Dr. Rae, having been nominated by the President, were by ballot elected Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "*Mahonia Aquifolia* as a Nurse of the Wheat Mildew (*Puccinia graminis*)." By CHARLES B. PLOWRIGHT, M.R.C.S. Communicated by Sir J. D. HOOKER, F.R.S. Received July 23, 1883.

It has always been difficult to account for the widely-spread nature of outbreaks of wheat mildew in districts in which the common barberry is either entirely absent or very uncommon. In the year 1874 the Rev. James Stevenson found at Glamis, in Forfarshire, an Æcidium upon *Mahonia aquifolia*, which the Rev. M. J. Berkeley* pronounced to be *Æcidium berberidis*. In the following year Dr. Paul Magnus† found the same fungus at Lichterfelde, near Berlin, but since that time it does not seem to have been noticed by any one. On the

* Berkeley and Broome, "Annals Nat. Hist.," Jan., 1875.

† Magnus, "Botan. Verein der Prov. Brandenburg. Sitzung.," 25 Juin, 1874. p. 76.

31st of May, 1883, Mr. William C. Little, of Stagsholt, March, gave me a freshly gathered specimen of *Mahonia aquifolia*, upon the berries of which the Æcidium was abundant. Knowing that upon the barberry no less than three different Æcidia occur, I determined to prove by direct experimental culture whether this one was the *Æcidium berberidis* of Persoon (the æcidiospore of *Puccinia graminis*). At 10 P.M. on the evening of the 31st May I placed some of the spores upon the cuticle of some wheat-plants which had been cultivated under a bell-glass. In eleven days the uredo of *Puccinia graminis* made its appearance upon these plants. The details of this, as well as of two other experiments, are appended. On the 13th June I placed some of the æcidiospores upon a piece of wheat cuticle; in twelve hours they had germinated, and a little later the germ-tubes were seen entering the stomata, in the same manner as those of *Æcidium berberidis* do (see figure). It is then clear that the Æcidium upon *Mahonia aquifolia* is identical with the *Æcidium berberidis* (Pers.), and is a part of the life-cycle of *Puccinia graminis*, and is unconnected with the *Æcidium magellanicum* (Berk.),* and the Æcidium of *Puccinia berberidis* (Mont.).†

The Mahonia in question is widely cultivated in gardens throughout England and is a favourite evergreen in shrubberies. It is also extensively planted in woods as a covert for game.

Experiment 151.‡—Four wheat seedlings, grown in a flower-pot, which have been continuously covered by a bell-glass since the seed was planted (at the end of April), had on 31st May the spores of the Æcidium on the fruit of *Mahonia aquifolia* applied to their green leaves. The bell-glass was replaced as soon as this was done. On the 10th of June on two leaves pustules of uredo were observed. On the 19th these plants were gathered. Every one of them had the uredo upon it.

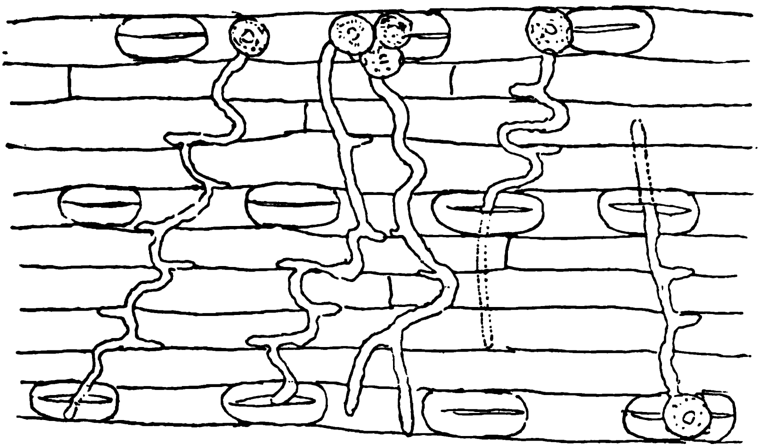
Six precisely similar wheat seedlings, planted at the same time and grown under the same conditions, as control plants, were perfectly free from uredo, and remained so up to the present time (19th June).

Experiment 152.—Three wheat-plants, grown by "water culture" in a room in the house, were removed from the bottle in which they were growing and planted in a flower-pot on the evening of the 31st of May. To their leaves the spores of the Æcidium on Mahonia were also applied as in the former experiment. On the 8th of June the presence of the mycelium of the uredo was indicated by the appearance of sickly yellow spots upon the leaves, and on the 10th the perfect uredo of *Puccinia graminis* was developed.

* Hooker, "Flora Antarctica," vol. ii, p. 450, Pl. 163, fig. 2.

† Montagne Gay, "Flor. Chil.," VIII, p. 46. "Sylloge," p. 314.

‡ The numbers refer to a series of experimental cultures made during the past three years upon the physiology of the Uredines.



Spores of Æcidium on *Mahonia aquifolia* germinating upon the cuticle of a wheat plant: showing the germ-tubes entering the stomata.

Experiment 162.—A wheat-plant, grown out of doors in the parish of West Lynn, was placed in a flower-pot in April. On the 31st of May this plant was removed to my garden, near King's Lynn, and covered by a bell-glass. The plant was far more robust and considerably larger than those employed in the former experiments. On the 2nd of June it was infected with fresh æcidiospores, sent by Mr. Little from Stagsholt. The bell-glass was removed on the 5th, and on the 10th of June the uredospores of *Puccinia graminis* made their appearance. On the 19th there were thirteen stems of this wheat-plant, about 18 inches in height; fourteen leaves were affected with uredo. There were many wheat-plants, of all ages, growing at this time in the garden, but upon no one of them did any *Uredo linearis* exist.

II. "Description of Teeth of a large Extinct (Marsupial?) Genus, *Sceparnodon* Ramsay." By Professor Owen, C.B., F.R.S. Received October 2, 1883.

(Abstract.)

In this paper the author describes teeth of a new genus of Mammal, representing a species of the size of the *Thylacoleo* or *Nototherium*, specimens of which teeth have been discovered in three distinct and remote localities in Australia. In shape the teeth resemble the scalpriform incisors of the upper jaw of the *Rodentia;* in the microscopic structure of the dentine there is a nearer resemblance to that in the incisor of the large extinct form of wombat (*Phascolomus*). Figures of the teeth, and of their dentinal structure mag-

nified, are appended to the text. The author remarks that the first indication of since restored species, *e.g.*, of the *Diprotodon*, as a large extinct Marsupial, was a portion of a tooth, and corresponding accessions of fossil remains may be expected to lead to a like reconstruction of the present animal. He is indebted to E. P. Ramsay, Esq., F.L.S., for casts of the first found specimens of the teeth in question to which the transmitter had appended the name *Sceparnodon*; subsequently the author received, through the kindness of C. H. Hartman, Esq., of Toouromba, Queensland, a large portion of the tooth itself.

III. "Evidence of a Large Extinct Monotreme (*Echidna Ramsayi*, Ow.) from the Wellington Breccia Cave, New South Wales." By Professor OWEN, C.B., F.R.S. Received November 3, 1883.

(Abstract.)

In this communication the author gives a description of a fossil humerus from the breccia cave of Wellington Valley, which repeats the characters of that bone in the existing monotrematous genus *Echidna* more closely than those of the same bone in any other known kind of mammal. The fossil, however, greatly exceeds in size that of the existing Australian species, *Echidna hystrix*, Cuv. The existence of, at least, two other kinds lately discovered living in New Guinea has been made known in memoirs by Professor Gervais and Mr. E. P. Ramsay, F.L.S.; these occupy, in respect of size, the interval between them and the Australian *Ech. hystrix*, but the subject of the present paper makes known the largest Monotreme hitherto discovered. Figures of the fossil in question, and of the corresponding bone of the smaller existing Australian kind, accompany the text. The fossil formed part of the series of remains obtained from the cave above cited, and was with them submitted to the author, who proposes to indicate the present acquisition by the name *Echidna Ramsayi*.

IV. "Correction to a paper 'On the Determination of Verdet's Constant,' published in the 'Phil. Trans.,' 1877." By J. E. H. GORDON, M.S.T.E. Communicated by Professor STOKES, Sec. R.S. Received October 5, 1883.

(Abstract.)

In revising my "Treatise on Electricity" for the second edition, in July, 1883, I noticed a discrepancy between the value of Verdet's constant obtained by myself and that deduced from M. H.

Becquerel's comparative experiments (see "Electricity," first edition, vol. ii, p. 235).

This led me to revise the calculations given in the "Phil. Trans.," 1877, and I see that in the final formula in that paper 2R has been used instead of R. This makes the value of the constant there given double its true value, which is—

$$\omega = 1{\cdot}52381 \times 10^{-5}.$$

This correction removes the discrepancy between my result and Becquerel's.

V. "Note on the Irregularities in Magnetic Inclination on the West Coast of Scotland." By T. E. THORPE, F.R.S., and A. W. RÜCKER, M.A. Received October 20, 1883.

In the Report of the results of the Magnetic Survey of Scotland, undertaken at the request of the British Association by the late Mr. Welsh during the years 1857 and 1858, it is stated by Professor Balfour Stewart (by whom the observations were reduced and the report drawn up) that the values of all the elements as determined in and adjacent to the Island of Mull were apparently largely affected by local attraction, and from a comparison of the various observations Professor Stewart was led to place the centre of the disturbance a little to the south of the Mull stations, and at a considerable depth below the surface. The effect of this local attraction was most apparent in the determination of the dip, which at Tobermory was upwards of 57′, and at Glenmorven, on the other side of the Sound, was 14′ in excess of the probable normal value, that is, the value unaffected by local disturbance and dependent merely on geographical position, as deduced by combining together all the other observations for Scotland, in the manner adopted by Sir Edward Sabine in discussing the observations of the previous Survey of 1836.

Dr. Stewart's localisation of the centre of disturbance was based partly on a consideration of the abnormal values exhibited by the observations made at the two stations on the Sound of Mull, and partly on certain irregularities manifested by the determinations taken on Islay and in Skye. So far as the Mull observations themselves were concerned, the clue as to the exact locality of the area of disturbance was of the very slenderest. Mr. Welsh appears to have made only a single observation of the dip at Tobermory; and although observations were made with two needles at Glenmorven, the divergence between the resultant values happens to be greater than is exhibited by any other pair of dip observations throughout the survey. Nevertheless, as we shall show, we are able to confirm

not only the general accuracy of Mr. Welsh's observations on this particular point, but also Professor Stewart's inference as to the probable *locale* of the area of disturbance.

Since the date of the publication of Professor Stewart's Report, the Island of Mull, and the adjacent district of Morvern, have been made the subject of very careful geological and petrological study, more particularly by Professor Judd (see his paper in the "Quart. Journ. Geol. Soc.," xxx, 220), who has demonstrated that the great central mountain group in the island is the denuded core of an immense volcanic pile, the base of which must have had a circumference of at least forty miles, and the lava streams from which must have flowed to enormous distances and in quantities so vast as to have accumulated to a thickness of 2,000 feet. We have in these huge accumulations of gabbro, dolerite, basalt, tachylite, &c.—all rocks containing more or less magnetite and, possibly, even finely-divided metallic iron, as in the basalts of the north of Ireland and of Greenland, an undoubted source of local attraction, sufficient in all probability to affect the needle to the extent observed by Mr. Welsh.

It seemed to us highly desirable, however, to repeat Mr. Welsh's observations in the light afforded by Mr. Judd's analysis of the geological features of the district. Although the geological facts doubtless serve to strengthen the validity of the magnetic observations, it should be stated that the direct evidence as to the existence of the great area of local attraction practically rested upon a single set of observations. It appeared to us, therefore, desirable, as a preliminary step to any subsequent survey of the west coast of Scotland, to confirm, in the first place, Mr. Welsh's observations as to the existence of the local attraction; secondly, to ascertain, if possible, the position of maximum disturbance; and thirdly, to determine the influence of the area on the direction of the isoclinal line.

Our observations were made with an excellent dip-circle belonging to Owens College, for the loan of which we are indebted to Professor Balfour Stewart. The instrument is known as "Dover No. 6"; it had already been employed by one of us in magnetic observations in America and in the Azores (see "A Magnetic Survey of the Fortieth Parallel in North America," "Proc. Roy. Soc.," vol. 30, 132; and "Note on the Magnetic Inclination in the Azores," "Proc. Roy. Soc.," vol. 31, 237). The circle is provided with two needles, each $3\frac{1}{2}$ inches long and 0·27 inch in maximum width. Similar precautions were taken to preserve the needles from rust as are indicated in the communications referred to, and the method of observation was the same as that previously adopted; it is identical with that described by Mr. Welsh in the "Admiralty Manual of Scientific Inquiry," third edition, 1859. With a single exception duplicate and independent observations were made in all cases with the two needles.

Our plan of investigation was briefly as follows:—In the first place we sought for a station sufficiently remote from the supposed area of disturbance, and itself free from any suspicion of local attraction, where we could hope to obtain a perfectly normal value for the inclination. We then proceeded to confirm this value by observations made at distances of a few miles and in different directions from this station on spots as free as possible from local attraction. Our normal station we found on the Island of Shuna in Loch Linnhe; this island is composed of alternations of micaceous schist and limestone, and a careful examination of the locality by our colleague Professor Green, to whom we are indebted for the geological notes which accompany our observations, revealed nothing which could exercise any disturbing influence on the dip. The subsidiary stations were near Carron Point in Loch Aber and on the shore of Loch Corrie. We then approached Mull along lines converging to the supposed place of maximum disturbance, making observations at tolerably regular intervals as we came towards the volcanic district as indicated on Professor Judd's map of Mull and Morvern. In this manner we imagined we should be able to determine the influence of the attractive mass upon the dipping needle. We next made observations near but not actually upon the extreme edge of the basalt in Morvern and on the opposite shore of the Sound; and lastly we made a number of determinations on Mull itself with a view of ascertaining, if possible, the position of the maximum disturbance.

The results are contained in the following table (see next page):—

These results serve, in the first place, to establish most unequivocally the existence of the area of local attraction which Dr. Stewart found to be indicated by the observations of Mr. Welsh. In the table showing the results of Mr. Welsh's observations, contained in the Report to the British Association, no particulars are given as to the exact spot in Tobermory on which the tripod was placed, nor is anything to be obtained on this point from Mr. Welsh's MS. notes of his survey work which are kept at the Kew Observatory. Nevertheless, when regard is had to the secular change in the inclination, our results are as concordant as could be expected. Nothing is, of course, known as to the precise amount of this change at Tobermory, but assuming that it is identical with that calculated by Dr. Stewart from the whole of the results of the surveys of 1836–7 and 1857–8, viz., an average yearly decrease of 2′·23, our value would become 72° 43′, which agrees closely with that observed by Mr. Welsh, viz., 72° 46′·8. This agreement would in all probability be actually closer if we assume that we have in Scotland as in England a progressive augmentation in the amount of secular change with an increasing westerly position.

Our observations further tend to show that the centre or focus of

Station.	Latitude.	Longitude.	Date 1883.	Greenwich mean time.	Needle.	Dip.	Mean dip.	Geological character of station.
	° ′ ″	° ′ ″		h. m.		° ′	° ′	
Island of Shuna, Loch Linnhe	56 34 56 N.	5 23 18·7 W.	Sept. 8	2 57 P.M.	1	70 56	70 54·5	Alternations of micaceous schist and limestone.
				3 55 „	2	70 53		
Near Carron Point, Loch Aber	56 43 30	5 15 45	„ 9	11 23 A.M.	1	71 08	71 08·5	Sand and gravel resting on quartz felsite. Hills around schist with syenite.
				12 12 P.M.	2	71 09		
Loch Corrie, Upper end	56 37 07	5 31 07	„ 11	10 26 A.M.	1	71 12	71 11·2	Granite: some basaltic dykes.
				11 16 „	2	71 10·5		
Kinloch Aline, Loch Aline	56 33 54	5 45 00	„ 12	11 31 „	1	71 12	71 09	Mica schist and gneiss. Just on the edge of the great spread of basalt which covers the north part of Mull and Morvern.
				12 8 P.M.	2	71 6		
Fishnish Bay, Sound of Mull	56 30 22	5 50 00	„ 12	4 5 „	1	70 27	70 27	Observations made on felsite but near the north margin of the great central intrusive mass of gabbro and basalt.
				4 45 „	2	70 27		
Bietich Island, Sound of Mull	56 37 04	6 02 45	„ 14	7 56 A.M.	1	70 58	70 58	Basalt.
Tobermory, Mull	56 36 59	6 03 50	„ 13	11 39 „	1	71 46	71 45	Well in the middle of the great basaltic spread.
				12 23 P.M.	2	71 44		
Loch Drumbuy, Morvern	56 38 54	5 56 05	„ 13	4 11 „	1	71 21	71 24	Mica schist and quartzite traversed by basaltic dykes: about half mile from the north boundary of spread of basalt.
				4 57 „	2	71 27		
Gobbar Island, Loch Ailort	56 50 55	5 46 55	„ 14	4 21 „	1	71 16	71 19	Mica schist and quartzite: a basalt dyke about 50 yards from place of observation.
				4 54 „	2	71 22		
Scriby Bay, Loch Tuadh	56 29 04	6 10 20	„ 18	9 5 A.M.	1	72 15·3	72 14·7	Basalt: to the west of the great central mass of gabbro.
				9 52 „	2	72 14·1		
Aird Kilfinichen, Loch Scridain	56 22 39	6 03 45	„ 19	10 1 „	1	70 41·4	70 40·2	Basalt.
				10 36 „	2	70 39		

Mr. Whipple, the Superintendent of the Kew Observatory, has been good enough to furnish us with a statement regarding the Kew magnetographs which includes the period from September 8th to the 19th, from which it appears that there were no movements of the magnets which could affect our observations on any of the above days.

the area of attractive matter in Mull is near the upper part of Loch Tuadh or Loch Na Keal; that is to say, at about the spot indicated by Dr. Balfour Stewart as its probable place. This spot is certainly very close to, and may indeed on a more complete examination turn out to be identical with the locality where Professor Judd places the great volcanic vent in the island.

The observations, moreover, show that great as may be the absolute mass of the magnetic matter in Mull, its influence extends to no very great distance from the island. The observations in Loch Aline to the north-east, and in Loch Scridain to the south-west, prove that the masses of igneous rocks, as these are constituted in Mull, may be approached to within a very few miles without appreciable effect on the needle. The determination of the normal direction of the isoclinal line in the neighbourhood of Mull is not likely therefore to be materially affected by this area of attractive matter. It is almost certain for instance, that the area was without sensible influence on the observations at Shuna, Carron Point, Loch Corrie, and Loch Ailort. At Loch Drumbuy the influence of the Morvern basalt is appreciable, whilst at Fishnish the effect of the horizontal component of the attractive force is very marked. Although this place is only about five miles in about the normal direction of the isoclinal from Kinloch Aline, there is a difference of 42′ in the dip, an amount of course far in excess of any possible error of observation.

Our observations lastly afford some indication of the probable amount of the secular change in the inclination in these parts. So far back as 1836·5, Sir Edward Sabine found the dip at Artornish to be 72° 43′; this place is close to Kinloch Aline where we this year found the dip 71° 9′; this is equal to an average yearly decrease of 2′. At our station in Shuna we observed the dip 70° 54′·5; the interpolated value for 1858 as obtained from the chart accompanying Professor Stewart's report is 71° 45′; this too is exactly equal to a yearly decrease of 2′. It is worthy of note that this value is identical with that adopted by Sir Edward Sabine in the reduction of all the Scotch observations made prior to 1858 to the mean epoch of 1842·5 ("Contributions to Terrestrial Magnetism," No. XII, "Phil. Trans.," 1870, p. 265).

Our observations are, of course, too few in number, and are distributed over too restricted an area to allow us to draw any definite conclusions as to the direction of the isoclinals at the present time, but it is almost certain that this direction will now be found to be very different from that observed by Mr. Welsh in 1858. It is to be hoped that before very long another magnetic survey of the whole of the British Isles will be set on foot. So far as Scotland is concerned the present time would appear to be very opportune for a new survey.

More than twenty-five years have elapsed since Mr. Welsh made his survey, and this was separated by an interval of twenty-one years from that which we owe to the joint labours of Sir Edward Sabine, Sir James Ross, and Mr. Fox. The instruments and methods of observation in 1858 were greatly superior to those of 1836-7, and hence a new survey made during the approaching period of minimum sun-spot disturbance, and on stations selected with careful reference to their geological character, would undoubtedly afford far more accurate data as to the absolute value of the magnetic elements and as to the extent of secular change in this part of the world than we at present possess.

VI. "On the Circulation of Air observed in Kundt's Tubes, and on some Allied Acoustical Problems." By Lord Rayleigh, D.C.L., F.R.S. Received October 23, 1883.

(Abstract.)

Experimenters in acoustics have discovered more than one set of phenomena apparently depending for their explanation upon the existence of regular currents of air, resulting from vibratory motion, of which theory has as yet rendered no account. This is not, perhaps, a matter for surprise, when we consider that such currents, involving as they do *circulation* of the fluid, could not arise in the absence of friction, however great the extent of vibration. And even when we are prepared to include in our investigations the influence of friction, by which the motion of fluid in the neighbourhood of solid bodies may be greatly modified, we have no chance of reaching an explanation, if, as is usual, we limit ourselves to the supposition of infinitely small motion and neglect the squares and higher powers of the mathematical symbols by which it is expressed.

In the present paper three problems of this kind are considered, two of which are illustrative of phenomena observed by Faraday.* In these problems the fluid may be treated as incompressible. The more important of them relates to the currents generated over a vibrating plate, arranged as in Chladni's experiments. It was discovered by Savart that very fine powder does not collect itself at the nodal lines as does sand in the production of Chladni's figures, but gathers itself into a cloud, which, after hovering for a time, settles itself over the places of maximum vibration. This was traced by Faraday to the action of currents of air, rising from the plate at the places of maximum vibration, and falling back to it at the nodes. In

* "On a Peculiar Class of Acoustical Figures; and on certain Forms assumed by Groups of Particles upon Vibrating Elastic Surfaces." "Phil. Trans.," 1831, p. 299.

a vacuum the phenomena observed by Savart do not take place, all kinds of powder collecting at the nodes. In the investigation of this, as of the other problems, the motion is supposed to take place in two dimensions.

It is probable that the colour phenomena observed by Sedley Taylor* on liquid films under the action of sonorous vibrations are to be referred to the operation of the aerial vortices here investigated. In a memoir on the colours of the soap-bubble,† Brewster has described the peculiar arrangements of colour accompanied by whirling motions caused by the impact of a gentle current of air. In Mr. Taylor's experiments the film probably divides itself into vibrating sections, associated with which will be aerial vortices reacting laterally upon the film.

The third problem relates to the air-currents observed by Dvorak in a Kundt's tube, to which is apparently due the formation of the dust figures. In this case we are obliged to take into account the compressibility of the fluid.

VII. "The Influence of Bodily Labour upon the Discharge of Nitrogen." By W. NORTH, B.A., F.C.S. Communicated by Professor J. S. BURDON SANDERSON, F.R.S. Received October 29, 1883.

(Abstract.)

The scope of this inquiry has been strictly limited to one question, viz., that of the influence of labour in modifying the normal relation between food and excreta. No attempt has been made to investigate the *mode* in which nitrogenous products come into existence in the organism.

The researches immediately bearing on the subject of this paper are those of Dr. Parkes ("Proc. Roy. Soc.," vols. 16 and 21), and those of Dr. Austin Flint, made on the pedestrian Weston ("New York Med. Journal," June, 1871). Dr. Parkes found that bodily exercise caused a slight increase in the nitrogen discharge during or immediately after labour. The increase was, however, so inconsiderable that it may well be questioned whether it could not be accounted for as dependent on the more perfect absorption of food; for although the diet of the soldiers experimented upon was carefully regulated, and the nitrogen it contained determined by analysis, with the result that before work the quantity of nitrogen taken in considerably exceeded the quantity discharged, the two became practically equal during the work period. Consequently if the whole period of observation is

* "Proc. Roy. Soc.," 1878. † "Edinburgh Transactions," 1866-67.

taken into account, the nitrogen discharged is found to be more than balanced by that of the food.

Dr. Austin Flint, on the other hand, found that over the whole period of work the excess of discharge was so large that no such explanation appeared to him admissible. If, however, comparison be made of the intake with the output of nitrogen during the whole time of observation, comprising three periods of five days each, before, during, and after labour, it is found that the two are unequal, the difference in *favour* of the nitrogen of the food amounting to 217 grs. in a total of 5075.

These results were subjected to careful experimental criticism in 1876 by Dr. Pavy ("Lancet," 1876, vols. i and ii), who showed as the result of his own analyses that the immediate effect of labour in increasing the nitrogen output is more than compensated by the concomitant and subsequent intake.

It is further to be considered that whatever results had been obtained by Dr. Austin Flint, they could not have been received without some misgiving, for his methods of research were insufficient as bases for quantitative statement. Thus the nitrogen of the *urine* was throughout determined by a process which is known to be liable to errors of variable amount, and which no care on the part of the worker is adequate to guard against.

So also as regards the intake of nitrogen. Dr. Flint's estimates were founded for the most part not on actual analyses of the material used, but on calculations based on the percentages given in M. Payen's tables ("Traité des Substances Alimentaires," 1865), which are known to be at best only approximately correct; moreover, the diet of Weston was of so complicated and variable a composition that, even if each constituent had been analysed, the result would still have been open to question.

The circumstances under which Dr. Austin Flint had to make his observations, probably made it impossible for him to secure uniformity of diet. In this respect the conditions of Dr. Parkes' experiments were immeasurably superior. Fully recognising that uniformity was essential, he fed his men in the simplest possible way; he was not, however, able to accomplish this without employing a diet which was so different from that to which, as soldiers, they were accustomed, that, however satisfactory its elementary composition might be, it could scarcely be considered as natural.

Notwithstanding this difficulty, the experiments of Dr. Parkes render it, to say the least, highly probable that the immediate effect of labour is to increase the discharge of nitrogen; they leave it undecided whether or not this increase occurs at the expense of stored material independently of any concomitant or subsequent increase of intake.

The decision of this point is the *object* of my experiments.

It will be obvious from the consideration of the experiments above mentioned, that in approaching this question, methods of research are of prime importance. These divide themselves very naturally under two heads—first, the diet, and secondly, the investigation of the excreta in order that the results may be satisfactory. It is essential first that the daily intake of nitrogen should be accurately known and admit of exact regulation, and secondly that the mode of analysis of the excreta should not be open to question.

The use of the method of combustion with soda-lime effectually disposes of the latter difficulty; the former is not so readily overcome.

It is a matter of common knowledge that the composition of the ordinary food-stuffs in their usual state is, from the chemist's point of view, exceedingly variable. The first consideration then is how to reduce them to such a state that this objection shall disappear.

The mode in which I have overcome this difficulty constitutes the chief difference between my experiments and those of other observers.

It would occupy too much space, and is indeed unnecessary, to describe in detail the process of preparation of the food-stuffs, and it will here suffice to say that I have acted on the principle that only fluids or powders can be accurately sampled and analysed; all my food-stuffs have been reduced to one of these states, and I may here enumerate them—

Meat (dried and ground to powder).
Flour.
Vegetables (dried).
Potato (Edward's patent desiccated).
Condensed milk.

All these articles can be obtained in quantity; all admit of being easily and accurately weighed and measured; they are in a state which readily admits of accurate analysis, and will keep indefinitely. Further they are the constituents of an ordinary mixed diet, and except in their palatability and mode of preparation, involve no serious departure from one's usual food.

By the use of these materials I can, knowing their composition, adjust the intake of nitrogen with the greatest possible nicety, and maintain it at any desired level for almost any length of time, besides having it in my power to use, not merely a similar, but precisely the same, diet at any time when it may seem desirable to repeat an experiment.

My plan of experiment was as follows:—

Believing as I do that previous food may materially affect the results of an experiment, and that the body has the power of storing nitrogen at one time which can be discharged at another as occasion

requires, I regulated my diet for some four or five days before beginning an experiment, and took rather more exercise than usual in order to get rid of any possible surplus in the body; in the latest experiments I have adopted the plan of abstaining from food on the first day of an experiment, in order to effect the discharge of this surplus, and I am inclined to regard this as the best method to pursue.

I then place myself upon a regulated diet of accurately known composition for nine or ten days, and about the middle of the period I perform a certain amount of muscular labour. In all experiments made thus far the labour consisted in walking a known distance, and carrying a load whose weight was accurately determined. By observing the relation of the nitrogenous intake to the output from day to day, I was enabled to determine with very considerable accuracy the effect produced by the labour upon it. In addition to the nitrogen the phosphoric and sulphuric acids were accurately determined in the food and excreta, and so I was able to institute a comparison, and observe the effect of the exercise in modifying the relationship.

It is impossible in a brief abstract to enter into the immense mass of detail which these experiments involve, but the following tables will, I think, suffice to exhibit the general nature of the results obtained.

Experiment I, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	135·33	15·03
" fæces	20·69	2·29
Total excreta	156·02	17·32
Total ingesta	158·78	17·64
Difference	2·76	0·32
P_2O_5 of urine	18·00	2·00
P_2O_5 of fæces	19·44	2·16
Total excreta	37·44	4·16
Total ingesta	34·84	3·87
Difference	2·60	0·29

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	14·15	15·74	+1·59
" fæces	2·48	2·15	−0·33
Total nitrogen	16·66	17·89	+1·23
P_2O_5 of urine	2·01	2·00	−0·01
P_2O_5 of fæces	2·54	1·85	−0·69
Total P_2O_5	4·55	3·84	−0·71
H_2SO_4 in urine	2·76	3 00	+0·24

Experiment II, 1882.

	Whole experiment.	Daily.
Nitrogen of urine	131·60	14·62
„ fæces	19·05	2·11
Total excreta	150·65	16·74
Total ingesta	158·78	17·64
Difference	8·13	0·90
P_2O_5 of urine	17·07	1·89
P_2O_5 of fæces	18·27	2·03
Total excreta	35·34	3·92
Total ingesta	34·84	3·87
Difference	0·50	0·05

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	13·77	15·29	+1·72
„ fæces	1·19	2·65	+1·46
Total excreta	15·22	17·95	+2·73
P_2O_5 of urine	1·97	1·83	−0·14
P_2O_5 of fæces	1·62	2·35	+0·73
Total excreta	3·59	4·19	+0·60
H_2SO_4 in urine	2·74	2·97	+0·23

Experiment III, 1882.

This experiment was begun and concluded by a fast of twenty-four hours; two sets of totals are therefore given, one including the first fast and one not including it.

	12 days.	Daily.	11 days.	Daily.
Nitrogen of urine	175·02	14·58	165·56	15·05
„ fæces	22·80	1·90	22·80	2·07
Total excreta	197·82	16·48	188·36	17·12
Total ingesta	194·06	16·17	194·06	17·64
Difference	3·76	0·31	6·30	0·52
P_2O_5 of urine	21·99	1·83	20·84	1·89
P_2O_5 of fæces	20·51	1·70	20·51	1·86
Total excreta	42·50	3·54	41·35	3·75
Total ingesta	42·59	3·55	42·59	3·87
Difference	0·09	0·00	1·24	0·12

Two days of work were introduced in this experiment, it is therefore to be considered as two experiments in one.

Experiment III, 1882.

1st Period.

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	12·92	13·47	+0·55
" fæces	1·43	2·14	+0·71
Total nitrogen	14·35	15·61	+1·26
P_2O_5 of urine	1·80	1·51	−0·39
P_2O_5 of fæces	0·95	2·01	+1·06
Total P_2O_5	2·75	3·52	+0·77
H_2SO_4 of urine	2·58	2·83	+0·25

2nd Period.

Daily.	Before work.	After work.	Difference.
Nitrogen of urine	13·23	18·27	+5·04
" fæces	1·83	2·43	+0·60
Total nitrogen	15·07	20·75	+5·68
P_2O_5 of urine	1·63	2·35	+0·72
P_2O_5 of fæces	1·56	2·40	+0·84
Total P_2O_5	3·19	4·75	+1·56
H_2SO_4 of urine	2·72	3·40	+0·68

The 2nd Period.

The whole of Period I is included in the time before work, although work was done.

The work done.

Experiment.	Distance walked.	Miles per hour.
I, 1882	30 miles	4·28
II, 1882	32 "	4·57
III, 1882—		
Period I	33 "	4·71
Period II	47 "	4·7

In each case the load carried was about 27 lbs.

Conclusions.

My results, while they confirm those of Dr. Parkes, show that the disturbance produced by very severe labour is much more immediate and of much greater intensity than that which Dr. Parkes observed, the explanation obviously being that in his experiments the exertion imposed on the soldiers who were made the subjects was inadequate.

I have further been able to show as the result of a very lengthy experiment in which several days of complete abstinence from food were introduced, and of which it is impossible in a short abstract to give the figures, that just as in Dr. Parkes' experiment on the effect of privation of nitrogenous food the diminution of the nitrogen stored in the system was followed by retention, *i.e.*, by a state of things in which the intake was greater than the output, so, after the disturbance of the nutrition of the body which is produced by severe labour, the immediate effect of which is to diminish the store of nitrogenous material in the system, there follows a corresponding diminution of discharge; so that the result is the same, viz., to use Dr. Parkes' own words "an insufficient supply at one time must be subsequently compensated" *whether the insufficiency be due to privation of food or to exercise.*

A third result of importance is this: that this storage of nitrogen is the expression of a tendency of the organism to economise its resources, which is much more constantly operative than has hitherto been supposed.

Finally, as regards the phosphates and sulphates, it has been shown that unless the exertion be very severe the phosphates are not increased, whilst the output of sulphates is distinctly increased in every case, the increase being in general terms proportional to that of nitrogenous material.

It is a matter of regret that the total sulphur of the food was not estimated; it is known that the percentage of sulphates contained in the food was insignificant as compared with that excreted in the urine, and consequently almost all the discharge must have been a product of oxidation.

I beg leave in conclusion, to state that the expenses of the present research, which have been extremely heavy, have been defrayed by a grant of the British Medical Association. I desire to express to the Association my most grateful thanks.

November 22, 1883.

Professor T. H. HUXLEY, President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election, was read as follows:—

President.—Professor Thomas Henry Huxley, LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Captain W. de Wiveleslie Abney, R.E.; Professor W. Grylls Adams, M.A., F.C.P.S.; the Duke of Argyll, K.T., D.C.L.; John Gilbert Baker, F.L.S.; Thomas Lauder Brunton, M.D., Sc.D.; William Henry M. Christie, Astron. Royal; Warren De La Rue, M.A., D.C.L.; Sir Frederick J. O. Evans, K.C.B.; Professor George Carey Foster, B.A.; Francis Galton, M.A., F.G.S.; James Whitbread Lee Glaisher, M.A.; Sir William Withey Gull, Bart., M.D.; Hugo Müller, Ph.D.; Professor Joseph Prestwich, M.A., F.G.S.; Professor Osborne Reynolds, M.A.; Osbert Salvin, M.A., F.L.S.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Formation of Ripple-mark in Sand." By G. H. DARWIN, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge. Received October 18, 1883.

The following paper contains an account of experiments and observations on the formation of ripple-mark in sand. The first section is devoted to experiments on the general conditions under which ripple-mark is formed, and especially on the mode of formation and maintenance of irregular ripples by currents. In the second section it is shown that regular ripple-mark in sand is due to a complex arrangement of vortices in oscillating water; and the last section gives some account of the views of certain recent observers in this field, and a

discussion of some phenomena in the vortex motion of air and water.

§ 1. *First Series of Experiments.*

A cylindrical zinc vessel, like a flat bath, with upright sides, 2 feet 8 inches in diameter and 9 inches deep, was placed on a table, which was free to turn about a vertical axis. Some fine sand was strewn over the bottom to a depth of about an inch, and water was poured in until it stood three inches deep over the sand. After some trials of simply whirling the bath, in which no regular ripple-mark was formed, I found that rotational oscillation with a jerking motion of small amplitude gave rise almost immediately to beautiful radial ripples all round the bath. If the jerks were of small amplitude the ripples were small, and if larger they were larger. On one occasion having made large ripple-marks, I oscillated the bath much more rapidly, and a second set of ripples sprang into existence in the furrows of the first set. Another time, when in consequence of irregularity in the motion, a set of radiating waves were generated in the water, a second set of transverse ripples were formed, which produced by interference a beautifully mamellated structure, arranged like a chess-board. In all these experiments the radiating ripples began first to appear at the outer margin of the bath and grew inwards; but the growth stopped after they had extended to a certain distance. If the jerking motion was violent, ripples were not formed near the circumference, and they only began at some distance inwards. After these preliminary trials, arrangements were made for regularising both the frequency and amplitude of oscillation of the bath. An attempt was then made to formulate the laws which govern the generation of ripple-marks. In the following notes of experiments the expression "octave" is used to denote a ripple-length which is one-half of the main or fundamental ripple, and the amplitudes are measured by the displacement of the edge of the bath.

The water stood 1 inch deep in the bath.

1. Amplitude 1 inch; frequency 52 per minute (complete oscillations).

No ripples formed after four minutes.

2. Amplitude $2\frac{1}{2}$ inches; frequency 52.

55 ripples round the circumference, extending about 4 inches inwards; somewhat irregular and with a tendency to break into the octave.

3. Amplitude $6\frac{1}{2}$ inches; frequency 52.

Motion very violent. About 5 large irregular ripples in the circumference, breaking at about 9 inches from the outside into about 40 ripples.

4. Amplitude $1\frac{3}{4}$ inches; frequency 52.

In one part 7 ripples to 9 inches; at another part 12 ripples to 1 foot; some tendency to break into the octave. The ripples only extended an inch or two from the edge.

More water was then poured in until it stood about $1\frac{3}{4}$ inches deep.

5. Repetition of No. 4.

There were 33 ripples in a half circumference (which is 22 inches); the ripples were more regular than in No. 4, with not so much tendency to break into the octave.

6. Water $2\frac{1}{2}$ inches deep; frequency 59; amplitude 3 or 4 inches.

43 ripples to the circumference (which is 44 inches), extending inwards 8 inches.

Hereafter the amplitudes were marked by a pointer projecting 8 inches from the edge of the bath.

7. Water $2\frac{1}{2}$ inches; frequency 75; amplitude $2\frac{1}{2}$ inches.

66 ripples to circumference, very regular, and extending inwards 4 or 5 inches.

8. Water $2\frac{1}{2}$ inches; frequency 74; amplitude 3 inches.

63 or 64 ripples, extending 6 or 7 inches; broken in two or three places.

9. Water $2\frac{1}{2}$; frequency 75; amplitude 4 inches.

53 ripples, extending 8 or 9 inches; not so regular.

10. Water $2\frac{1}{2}$; frequency 78; amplitude 5 inches.

47 ripples, extending 10 or 11 inches.

11. Water $2\frac{1}{2}$; frequency 75; amplitude 6 inches.

Agitation violent; all the coarser sand collected round the margin, without ripple-mark for 4 inches inwards; from 4 to 11 inches inwards, rather irregular ripples about 37 to circumference; the usual flat centre.

12. Water $2\frac{1}{2}$; frequency about 80; amplitude 7 inches.

The water churned up the sand with violence; margin the same as No. 11; from 8 to 12 inches from margin rather irregular ripples, about 34 to circumference.

13. (Bad observation.) Water $2\frac{1}{2}$; frequency about 85; amplitude about $1\frac{1}{2}$ to 2 inches.

80 ripples to circumference.

14. (Bad observation.) Water $2\frac{1}{2}$; frequency 57; amplitude 2 inches.

No ripples raised.

An analysis of the observations marked 7 to 14 was made on the hypothesis that the water remained still, when the bath oscillated with a simple harmonic motion. I endeavoured to find whether λ, the wave-length of ripple (in inches) was directly proportional to v, the maximum velocity of the water relatively to the bottom (in inches per minute) during the oscillatory motion; also the values of v_1 and

v_2, the least and greatest velocities of the water compatible with the formation of ripple-mark.

The following are the results:—

$\lambda \div v$.		Feet per sec. v_1.		Feet per sec. v_2.
·0031	....	·51 to ·56	....	—
·0027 to ·0028	....	·46 to ·51	....	—
·0024	....	·43 to ·49	....	—
·0021	....	·43 to ·52	....	—
·0023	....	·50	....	1·2
·002	....	·56	....	1·12
—	....	more than ·42	....	—
Mean ·00245 min. or ·147 second.	....	·503	....	1·2

It appears therefore that ripples are not formed if the maximum velocity of the water relatively to this particular sand, estimated on the above hypothesis, is less than half a foot or greater than a foot per second; and that if v be that maximum velocity in inches per minute, the wave-length of ripples generated is ·00245v, or ·147v when v is measured in inches per second. The results seem as fairly consistent with one another as could be expected. It will appear from section 2 that the maximum velocity of the water, as estimated on the hypothesis that the water as a whole executes a simple harmonic oscillation relatively to the bottom, does not give the maximum velocity of the water in contact with the sand relatively thereto. The quantity called v is not in reality the maximum velocity of the water in contact with the bottom relatively thereto, but it is in fact 6·283 times the amplitude multiplied by the frequency. Thus we cannot conclude that a current of half a foot per second is just sufficient to stir the sand. In the state of oscillation corresponding to v_1, it is probable that part of the water at the bottom is moving with a velocity much greater than half a foot per second relatively to the sand. The number of the experiments analysed is insufficient for the accurate determination of the law connecting wave-length, amplitude, and frequency; but this branch of the subject was not pursued further because other observers, whose work is referred to in § 3, have made a number of experiments with this object.

It was after making this set of experiments that I hit on what appears to be the key-note of the whole phenomenon.

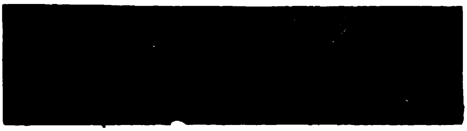
A series of ripples extending inwards for some distance having been made by oscillation, and the water having come to rest, the bath was turned slowly and nearly uniformly round. The ripples were then observed to prolong themselves towards the centre; this shows that a uniform current is competent to prolong existing ripples. The

uniform current flattened the tops of the ripples, but made the lee-side steeper. After being exposed to a prolonged current, the ripples were not only not in course of obliteration, but became somewhat more pronounced.

The sand was then smoothed with the edge of a board; after the exposure of the sand to a current, marks made with the edge, which at first were too faint to be seen, became by a course of development well-defined ripples. The whole surface became gradually mottled with irregular chains of ripples of which the weather-side was a very gradual slope, and the lee-side was steep. The appearance was strikingly like that of drifted snow. As it might be conjectured that there would be eddies or vortices on the lee-side, I made some regular ripple-marks by oscillation, and then exposed them to a current. I shortly observed minute particles lying on the surface of the sand climbing up the lee-slope of the ripples apparently *against* stream. This proved conclusively the existence of the suspected vortices.

If when the bath was at rest, a sudden motion was given in one direction, the sand on the lee-side of each ripple was observed to be churned up by a vortex. By giving a short and sudden motion, I was able to see the direct stream pile up the sand on the weather-side and the vortex on the lee-side. Fig. 1 shows the effect of a

Fig. 1.



single short jerk. Two little parallel ridges of sand were formed, namely (a) by the direct stream, and (b) on the lee-side by the vortex, a little below the crest of the ripple-mark.

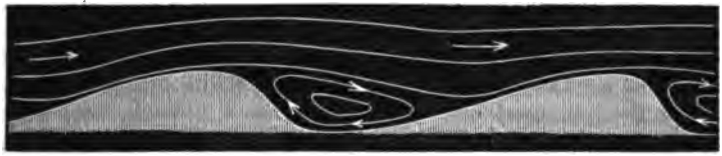
It is thus clear that casual surface inequalities are accentuated by the combined action of the direct stream and of the vortex.

For the purpose of examining the vortices a glass tube was drawn out to a fine point, and fitted at the other end with a short piece of india-rubber tube. With this a drop of ink could be squirted out at the bottom of the water. This method was adopted in all subsequent observations, and it proved very valuable. It may be worth mentioning that common ink, which is heavier than water, was better than aniline dye; and the addition of some sulphate of iron to the ink improved its action.

A drop of ink was placed in the furrow between two ripples; as soon as the continuous stream passed, the ink was parted into two portions, one being sucked back apparently against stream up the lee-side of the ripple-mark, and the other being carried by the direct

stream towards the crest. By observing the limits of the transport of the ink, I conclude that the stream lines were as shown in fig. 2.

Fig. 2.



These points being settled, it remained to discover how the vortices were arranged which undoubtedly must exist in the oscillatory formation of regular ripples. The rapidity of the necessary oscillations made this a task of some difficulty.

§2. *On the Formation of Ripple-mark by Oscillation.*

The observations were made in two different ways.

In the first of these, which also ultimately proved to be the most successful, the ripple-mark was made in a glass trough about 1 foot long, 5 inches wide, and 6 inches deep. In order to observe the formation of the ripples absolutely in profile, a sheet of glass fitting the trough was placed to stand on four short corks at the bottom. The trough was then put to rock on two corks, one at each side, on the line which bisects its length. Two other slightly shorter corks were put at the ends; these served as stops, and only allowed it to rock through a very small angle. The trough was placed on a window-sill with a strong light outside, and was gently rocked by hand.

When the trough is half filled with water, and sand is sprinkled on the glass plate, it is easy to obtain admirable ripple-marks by gently rocking the trough.

When a very small quantity of sand is sprinkled in and the rocking begins, the sand dances backwards and forwards on the bottom, the grains rolling as they go.

Very shortly the sand begins to aggregate into irregular little flocculent masses, the appearance being something like that of curdling milk. The position of the masses is, I believe, solely determined by the friction of the sand on the bottom, and as soon as a grain sticks, it thereby increases the friction at that place.

The aggregations gradually become elongated and rearrange themselves. As soon as the formation is definite enough to make the measurement of the wave-length possible, it is found that the wave-length is about half of what it becomes in the ultimate formation.

Some of the elongated patches disappear, and others fuse together and form ridges, the ridges then become straighter, and finally a regular ripple-mark is formed with the wave-length double that in the initial stage.

When a drop of ink is put on the glass without any sand, it simply slides to and fro, with perhaps a faint tendency to curdle, but it cannot be caused to form ripple-mark. This shows that the initial stage when the sand is beginning to curdle is due simply to friction.

When the ink is put upon a flocculent mass it betrays some kind of dance in the water, but the layer of water disturbed is so thin that it would hardly have been possible to detect the law of the motion from this case alone. When, however, the nature of that motion, as described below, has been discovered, the same kind of motion may be recognised in the dance of the ink over these flocculent aggregations.

I found in the later experiments that it was advantageous to have a very regular ripple-mark. I therefore sprinkled sand on the sheet of glass, and, before beginning the rocking, I traced regular furrows in it with the point of my finger. A few oscillations of the trough soon effaced all signs of the artificial origin, and the ripple crests with the bare glass in the furrows, were absolutely indistinguishable, except by perfect regularity, from those produced naturally. Most of the observations were, however, made with the natural ripples, and it was only towards the end that I adopted this plan in order to save time and to obtain perfect regularity.

In the rocking trough, the water moves whilst the bottom of the vessel is still, save for the small rocking motion. A second arrangement was, however, made in which the converse is true. A sheet of plate glass is caused to oscillate in the bottom of a trough with glass sides. The oscillator is moved by a connecting rod and crank driven by a small water-motor, the throw of the crank is small, and the rapidity of oscillation can be varied within considerable limits.

When sand is sprinkled on the oscillating sheet of glass, phenomena such as described above are again observed, and good regular ripple-mark is formed in the sand. Although much was learned from this instrument, still the rocking trough was on the whole more useful.

It appeared to be certain from the first set of experiments that ripple-mark was due to eddies or vortices, and the question remained as to how the vortices were arranged in oscillatory motion. It required some practice, and many hours of watching, to establish the conclusions explained below, indeed the phenomena next described were only detected long after that which follows them in this paper.

If a very gentle oscillation be started, the layer of ink on the crest of a ripple-mark becomes thicker and thinner alternately, swaying backwards and forwards; then a little tail of ink rises from the

crest, and the point of growth oscillates on each side of the crest; the end of the tail flips backwards and forwards. Next the end of the tail spreads out laterally on each side, so that a sort of mushroom of ink is formed, with the stalk dancing to and fro. The height of the mushroom is generally less than a millimetre.

FIG. 3.

Fig. 3 is the best representation I can make of this appearance, which I shall call an ink mushroom. The first of these figures gives the extreme of excursion on one side, the second the mean position, and the third the extreme on the other side. The figures show the state of affairs when the oscillation is very gentle, so that the amplitude of oscillation of the main body of water is small compared with the wave-length of the ripple-mark. The elongated hollows under the mushroom are the vortices, and the stem is the upward current. If the ink be thick these spaces are clouded, and the appearance is simply that of an alternate thickening and thinning of the ink on the crest. When one is familiar with this motion, after examining it carefully with gentle oscillation over ripple-mark of some size, the same kind of dance may, I think, be detected in the stage of ripple manufacture after the sand has curdled into elongated flocculent masses.

The oscillations being still gentle, but not so gentle as at first, streams of ink from the two mushrooms on adjacent crests creep down the two slopes into the furrow between the adjacent ridges, and where they meet a column of ink begins to rise from the part of the water whose mean position is in the centre of the furrow. The column is wavy, and the appearance is strikingly like that of smoke rising from a fire in still air.

The column ascends to a height of some 5, 10, or perhaps 20 times the height of the ripple-marks, according to the violence of the agitation. It broadens out at the top on each side and spreads out into a cloud, until the appearance is exactly like pictures of a volcano in violent eruption; but the broad flat cloud dances to and

fro relatively to the ascending column. The ink continues to spread out laterally and begins to fall on each side. In this stage if the ink is not thick it is often very like a palm-tree, and for the sake of a name I call this appearance an ink tree. The branches (as it were) then fall on each side, and the appearance becomes like that of a beech-tree, or sometimes of an umbrella. The branches reach the ground, and then creep inwards towards the stem, and the ink, which formed the branches, is sometimes seen ascending again in a wavy stream parallel to the stem.

Perhaps a dozen or twenty oscillations are requisite for making the ink go through the changes from the first growth of the tree.

The descending column of a pair of trees comes down on to the top of the mushroom. I have occasionally, when the oscillations are allowed to die, seen both tree and mushroom, but the successful manufacture of the tree necessitates an oscillation of sufficient violence to render the observation of the mushroom very difficult.

The alternate thickening and thinning of the ink on the crests seems to render it probable that with moderate oscillation the mushroom vortices are still in existence, or at any rate that alternately one and the other is there. With violent oscillation, when the stem of the tree is much convoluted, as described below, it cannot be asserted that the mushroom vortices exist, and I am somewhat inclined to believe them to be then evanescent.

Each side of the ink tree is clearly a vortex, and the stem is the dividing line between a pair, along which each vortex contributes its share to the ascending column of fluid. The vortex in half the tree is clearly in the first place generated by friction of the vortex in its correlated mushroom, and is of course endued with the opposite rotation. The ascending stem of the tree is a swift current, but over the mushroom the descending current is slow until close to the mushroom, when it is seen to be impelled by pulses.

I was on one occasion fortunate enough to observe a mote in the water which was floating nearly in the centre of a tree vortex, and counted twelve revolutions which it made before it was caught away from its fortunate position.

If the adjoining crests are of unequal height the stem of the tree is thrown over sideways away from the higher crest; and indeed it requires care to make the growth quite straight. The ink in the stem ascends with a series of pulses, and it is clear that there is a pumping action going on which renders the motion of each vortex somewhat intermittent, the two halves of the tree being pumped alternately.

The amount of curvature in the stem of the tree depends on the amplitude of the oscillation of the water. Figs. 4, 5, 6, give fair representations of ink trees.

FIG 4.

FIG 5.

FIG. 6.

Fig. 4 is the palm-tree stage with gentle oscillation, and figs. 5 and 6 represent the appearance when the amplitude is greater.

Fig. 7 exhibits a tree in which the growth is one-sided on account of inequality between the heights of the bounding crests.

FIG. 7.

Fig. 8 represents a mushroom and a tree which I have occasionally succeeded in observing simultaneously.

Fig. 8.

The ink is propagated along the convolutions of the stem of the ink tree, but the convolutions are themselves propagated upwards, and each convolution corresponds to one oscillation. The motion of the ink along the convolutions soon becomes slow, but the convolutions become broader and closer. Thus the upper part of the tree is often seen to be most delicately shaded by a series of nearly equidistant black lines. A perfectly normal ink tree, made by a very thin stream of ink, would be like the fig. 9, in which the whole is formed by a

Fig. 9.

single line; but it is not possible to represent the extreme closeness of the lines adequately.

In the transition from the mushroom stage to the tree stage it appeared to me that it was very frequent that only half the ink tree was formed. At any rate I have frequently noted the mushrooms and half the tree vortices lasting during many oscillations, and then the other halves of the trees gradually appeared. This might, of course, be due to an accidental deficiency of ink in an invisible tree vortex, but I have observed this appearance frequently when there is ink at the stem of the tree, and when there seemed no reason why it should only be carried up in one ascending stream and not in the other.

If the agitation is very gentle the sand on the crests of the ripple-marks is just moved to and fro; with slightly more amplitude, the dance is larger, and particles or visible objects, such as minute air-bubbles, in the furrows, also dance, but with less amplitude than those on the crests. When the rocking is gentle the oscillation in the

furrow appears to be in a different phase from that on the crest, with more violent rocking I did not observe the difference of phase. The dance is not a simple harmonic motion like that of the main body of the water relatively to the bottom, but the particles dash from one elongation to the other, pause there, and then dash back again.

As the amplitude further increases the furrows are completely scoured out, and the sand on the crests is dashed to and fro, forming a spray dancing between two limits. With violent agitation this dance must have an amplitude of more than half a wave-length. If the agitation be allowed to subside the dance subsides, and when the water is still the ripple-mark is left symmetrical on both sides. With extremely violent oscillation all the water becomes filled with flying dust, and it is no longer possible to see what is happening. This seems to be the condition when the agitation is too strong for the formation of ripple-mark. It is probable that the rush of water sweeps away the existing ripple-mark, and there is then no longer anything to produce a systematic arrangement of vortices.

In fig. 10 I have tried to exhibit the dance of the vortices by the succession of figs. i to vii. When the amplitude of oscillation is the same as when the ripple-mark is generated, the series of changes is of the kind shown. The figures succeed one another in time, but they do not pretend to such accuracy as to represent the stages at rigorously equal intervals.

The dotted waves show a mean contour of the ripple-mark; they are introduced to show the displacements of the crests relatively to the mean position. A perfectly symmetrical ripple-mark does not, however, present a simple harmonic outline, for the hollows are flat and the crests rather sharp.

The convoluted line is the stem of the ink tree, which forms the dividing line of the two vortices; the curved arrows show the direction of rotation.

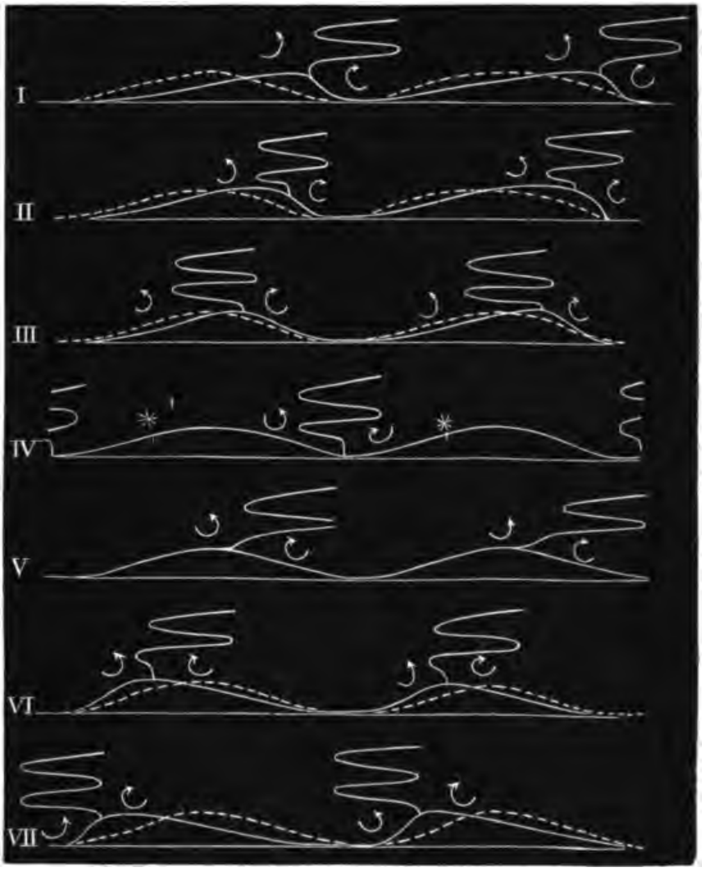
In i the water is at its elongation to the right. The crest of the ripple-mark is also at its extreme to the right, and the right hand slopes are steep, whilst the left are gentle. Here the water is at rest except for the vortices, which both tend to carry sand up to the crest.

In ii the general mass of water is beginning its movement to the left; it carries with it the upper convolutions of the ink tree, but leaves the root very nearly in the same position as in i. The crest of the ripple-mark is but little displaced. In iii the crest has begun its displacement, so that although the root of the ink tree has begun moving to the left, it is still over the crest. The convoluted stem continues to move to the left with increasing velocity, leaving the root behind it over the crest.

Just before the convolutions reach the position over the middle of

the furrow, the root leaves its crest and moves with very great speed to the left. In iv the root is just passing under the convolutions. The whole system is moving with its maximum velocity, but the root outstrips the stem. The two slopes of the crest are nearly symmetrical. In v the root has gained so far on the convolutions as to have again reached a crest.

FIG. 10.



In vi the convolutions have caught up the root, and the crests are being displaced.

Finally vii is a repetition of i in the opposite direction, and the half oscillation is completed.

If in these figures i to vii we take the wave-length of ripple-mark as unity, the amplitude of oscillation of the main body of water is 2·1,

that of the crests is ·7, and the breadth of the convolutions of the tree is ·3. The sum of the amplitude of oscillation of the crest, with the breadth of the convolutions, and the wave-length of ripple-mark is equal to 2, and this is very nearly equal to the amplitude of oscillation of the water, as it ought to be.

The law which governs the intensity of the vortices must be a matter of inference, since I found the motion too rapid to be sure of anything save that the vortices are driven alternately by pulses, and that the motion was most energetic near the elongations.

In i the right hand vortex of a pair must be at its maximum of intensity, and it seems probable that the left hand vortex has a sub-maximum in consequence of the friction of the water along the dividing line. During the return motion from i to vii, the left hand vortex must be increasing in intensity, so that it is at its maximum in vii. Probably the right hand vortex diminishes in intensity from i to v, and then increases to its sub-maximum in vii.

I am not able to say from observation that the vortices which have been described as giving rise to the ink mushrooms actually exist in this state of oscillation, but if they are there, one of them should be found at the point marked with an asterisk in iv.

The figures tell better than words the mechanism by which the ripple-mark is made and maintained, and the cause of the dance of the crests. The only difficulty is in stage iv, where the root of the tree is in the state of transferenee from one crest to the next. In this stage the vortices would seem to be in the act of degrading the ripple-mark, but they are not then either of them at their maximum of intensity, and the time during which this holds good is exceedingly short compared with the whole semi-period of oscillation. It seems somewhat likely that small vortices are called into existence at the points marked with asterisks in iv, which serve to protect the ripple-marks from degradation during the transference.

Fig. 11, i to vii, exhibits the dance of the vortices when the oscillation of the water is considerably less in amplitude than the wave-length of ripple-mark.

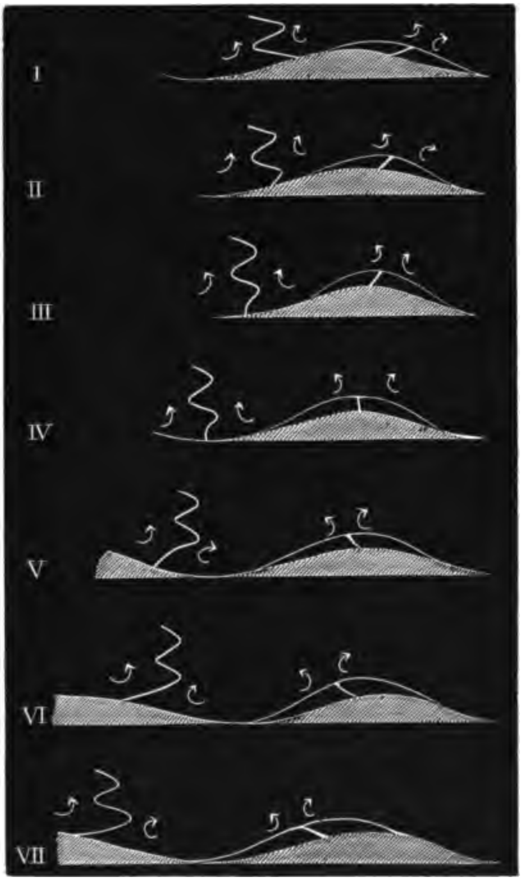
Here the crests of the ripple-mark are scarcely sensibly disturbed. Above the crest is drawn the pair of mushroom vortices, the curved arrows showing the direction of rotation being placed outside of the mushrooms; but I am not able to satisfy myself that they are both in existence during the whole oscillation. Fig. 8 above exhibits an appearance which I have sometimes seen, which seems to show that they may both exist together with an ink tree.

We must now draw attention to the manner in which the convolutions are added to the ink tree, and thus show the continuity of this fig. 11 for gentle oscillation with fig. 10 for violent oscillation.

In fig. 10, in ii, iii, iv, a convolution is added, which is unwrapped

again in v. It is again formed in vi and vii, and then becomes permanent, and is transmitted up the stem of the tree.

FIG. 11.



In fig. 11 the convolution is added in ii, and then remains permanently part of the tree; but a partial convolution is added in iii and iv, which is unwrapped again in v, vi, vii.

Thus in violent oscillation the convolution is permanently added just before an elongation, and in gentle oscillation afterwards. It would be easy to construct a figure for an intermediate amplitude in which the convolution is added just at elongation. If the oscillation gradually increases, the convolutions are permanently added sooner and sooner, and at the same time the formation of convolutions and subsequent unwrapping assumes more and more prominence.

It must be understood that these figures are drawn from the results of long watching of the process. My attention was at one time directed to one part of the phenomenon, and at another to a different part, and the amplitudes were constantly varied. I do not pretend to be able to see all these changes in a single half oscillation, lasting barely half a second. It may appear that I am incorrect in some parts of the construction, but I would ask any one who repeats the experiments not to condemn me hastily, for the constructions which I have given are the results of frequent trials and errors in the attempt to represent the changes observed.

I have not been able to determine exactly the mode of motion in the initial stages of ripple-making, when the oscillation has large amplitude, but when the ripple-marks are still in what I have described as the curdling stage.

If a current be passed over existing ripple-mark a vortex is established on the lee of each ripple; if the current be reversed the vortex is on the other side. Thus intermittent opposite currents will form ripple-mark, but probably without giving it a very regular wave-length.

If the intermittence is rapid, the vortex established on the lee-side, when the current is in one direction, is not annulled when the current is reversed, but it will be carried over the crest of the ripple-mark, and will diminish in intensity, whilst the new vortex with opposite rotation is established.

The study of a very gentle oscillation over existing ripple-mark, by means of a drop of ink placed on the ridge, enables us to observe these vortices (see fig. 3). I think it depends on the amplitude of oscillation whether both vortices are always in existence and simply vary in intensity, or whether the vortex due to motion to the right is quite annulled during the motion to the left, and *vice versâ*.

It may be suspected, therefore, that, in the early stages of ripple-making, when the amplitude of oscillation is large, vortices are set up in the lee of each aggregation of sand, in the same way as if the current were permanent, and that when the current is reversed these vortices are speedily annulled, and a new set on the other side of the aggregations is established. When a drop of ink is put on an aggregation, and the oscillation is started, the ink forms a layer of not more than half a millimetre in thickness. It is easy to see that there takes place some kind of rapid oscillation which is not simply harmonic. It appears to present all the characters of the motion when gentle oscillation is established over ripple-mark of some height, and therefore it is probable that the motion is of the same kind in both cases.

When the aggregations are more pronounced, small correlated tree vortices are set up. As above stated, it has seemed to me that frequently only half of each tree vortex is set up at first.

I am disposed to regard this as the transitional state from the mode of oscillation, which produces the octave with small height of ripple-crest, to the fundamental with considerable height.

In gentle oscillation over high ripple-mark the tree vortices are, in the first instance, seen to be started by the mushroom vortices, and the same is probably true of the condition we are considering.

If the suggested view as to the mode of transition be correct, then we must suppose that at first every alternate tree vortex is started by its correlated mushroom vortex. If there be no tree vortices, or if there be only every alternate one, the vortices can pack twice as close as if the trees are symmetrical; but the existence of a half tree vortex tends to generate its other half, and this half cannot exist normally unless every alternate ripple-mark is removed. The degradation of the alternate ripple-mark must arise then from the existence of the second half of the ink tree. In these early stages the phenomenon is not highly regular, and therefore, besides the smallness of the scale and the rapidity of the motion, we have the difficulty of irregularity to contend with.

Other observers have endeavoured to determine the laws connecting the wave-length in the ultimate formation with the various concomitant circumstances, and I shall leave this subject to the following section, where some account of their work will be given.

We may summarise the results of these observations as follows :—

The formation of irregular ripple-marks or dunes by a current is due to the vortex which exists on the lee of any superficial inequality of the bottom; the direct current carries the sand up the weather slope and the vortex up the lee slope. Thus any existing inequalities are increased, and the surface of sand becomes mottled over with irregular dunes. The velocity of the water must be greater than one limit and less than another, the limiting velocities being dependent on the average size and density of the particles. Existing regular ripple-mark is maintained by a current passing over it perpendicular to the ridges. A slight change in form ensues, the weather slope becoming less steep and the lee slope steeper. The ridges are also slowly displaced to leeward. The regular ripple-mark may also thus be somewhat prolonged, so that although a uniform current cannot, as I believe, form regular ripple-mark, yet it may increase the area over which it is to be found.

Regular ripple-mark is formed by water which oscillates relatively to the bottom. A pair of vortices, or in some cases four vortices, are established in the water; each set of vortices corresponds to a single ripple-crest and the vortices oscillate about a mean position, changing their shapes and intensities periodically, but not with a simple harmonic motion.

The successive changes in the vortex motion, whilst ripple-mark

is being established, and when the amplitude of oscillation over existing ripple-mark varies, are complex. As far as I have been able to determine, the following is an account of the phenomena:—

We begin with variation in amplitude of oscillation over existing regular ripple-mark, where the height of the undulations is not a very small fraction of the wave-length.

When the amplitude of oscillation is small compared with the wave-length, a pair of small vortices are established above the crest of each ripple-mark, rotating in opposite directions. In the mean position the upward current is over the crest, and the current of water tends to carry up sand from each furrow to the crest. The dividing line of the vortices oscillates, but the bottom of the line has much less amplitude of oscillation than the top, so that the dividing line is alternately inclined to one and the other side of the vertical. The vortices are thus carried backwards and forwards over the crest of the ripple, but the current always tends to maintain the crest, merely displacing very slightly the position of the highest point. The vortex which is on the lee-side is more intense than the other. We will call these the primary vortices. (See fig. 3.)

Suppose now the amplitude of oscillation to be somewhat larger; then the primary vortices by their friction on the adjacent water generate two other vortices. The upward current in these secondary vortices has its mean position over the middle of a furrow, and the current comes down immediately over the upward current of the primary pair of vortices. It appears that sometimes only every alternate one of the secondary vortices is established. The upward current of the secondary vortices oscillates with a motion which is very far from being harmonic. It remains at its elongation for a long time and then darts across to the other elongation. (See fig. 11.) During this mode of oscillation the primary vortices are carried much further backwards and forwards over the crests.

With still larger amplitude of oscillation it is no longer possible to distinguish the primary vortices, and the secondary vortices increase in intensity. It seems probable that the primary vortices are no longer both in existence during the whole oscillation, but that they are alternately created and annulled, so that when one exists the other does not. If this be so the vortex which exists is that which is on the lee side of the ripple in the state of motion at the instant.

With strong oscillation the secondary vortices apparently do all the work, and the primary vortex, if it exists, only exists for a short time, whilst it may serve as a protecting vortex to the ripple-crest, during the rapid transference of the dividing line of the secondary vortices from one crest to the next. Each secondary vortex is alternately a vortex under the lee of a ripple-mark as exhibited in fig. 10. Mere description is hardly sufficient to explain the motion.

With very violent oscillation the ripple-marks are obliterated, and the water is filled with flying dust.

We now revert to the initiation of ripple-mark.

If the surface be very even, as when sand is sprinkled on glass, when a uniform oscillation of considerable amplitude be established, the sand is carried backwards and forwards and some of the particles stick in places of greater friction. As soon as there is any superficial inequality, it is probable that a vortex is set up in the lee of the inequality which tends to establish a dune there. Such vortices are, however, too small to be seen. The return current in the second half of an oscillation maintains the dune, a vortex being established on the other side, now the lee-side. As the sand tends to stick by friction in a great number of positions, the sand agglomerates into elongated patches, and the patches are so near to one another that the vortex on either side of one patch just fails to interfere with the next patch. As the patches elongate and regularise themselves the vortices increase in intensity, and the vortex established on one lee is not obliterated in the return current. The two vortices are then the primary vortices described above. As the ripple increases in height by the obliteration of some of the elongated patches, the primary vortices set up the secondary vortices. Perhaps the normal state of transition is that only one of the secondary vortices is established at first, and that when the other secondary vortex is set up it tends to obliterate every alternate ripple-mark, and thus to generate a ripple of double wave-length. As the ripples increase in height the secondary vortices become more and more important, and the primary less important. The final or stationary condition is that described above as the case of strong oscillation.

It is to be admitted that this history of the successive stages of the formation of ripple-mark is to some extent speculative, but it is the only method of formation which appears to accord with the various phenomena observed and described above.

It is important to note that when once a fairly regular ripple-mark is established, a wide variability of amplitude in the oscillation is consistent with its maintenance or increase. No explanation of ripple-making can be deemed satisfactory which does not satisfy this condition.

In this summary no attempt has been made to go over again the various peculiarities of the motion, which have been noted above, such as the dance of the crests and of the convolutions of the dividing line of the vortices. We must refer the reader back for the consideration of these points.

§ 3. *The Work of previous Observers and Discussion.*

Some valuable papers on ripple-mark have been lately published.

The first of these is by Mr. A. R. Hunt.* In it he makes an extensive collection of observations on the natural history of ripple-mark. As, however, he does not touch at any length on the mode of formation, I have but little to say on his work. He remarks that regular ripples are due to alternating currents, and that the irregular marks due to currents ought to be distinguished by another name from the regular marks formed by oscillating water. M. Forel, whose paper is referred to below, takes the same view, and describes these irregular marks as dunes. My own observations seem to accord well with the facts collected by Mr. Hunt.

The second paper is by M. Casimir de Candolle.† His experiments have led him to enounce (p. 245) the following general law:—

"When a viscous material in contact with a less viscous liquid experiences an oscillatory or intermittent friction, arising from the relative motion of the liquid layer, 1st, the surface of the viscous material is rippled perpendicularly to the direction of motion; and 2nd, the wave-length is directly proportional to the amplitude of the oscillation."

The word viscous cannot here have its usual meaning, for sand cannot be called viscous. The epithet seems to denote that the constituent parts of the material are mobile, and that there is a considerable amount of internal friction.

When oscillations are set up in a vessel containing two fluids of very unequal viscosity, such as tar and water, ripples are formed on the more viscous fluid. But if the two fluids do not differ widely in viscosity, as mercury and water, water and turpentine, essence of cinnamon and water, ripple-mark is not generated. If, however, a layer of powder be introduced at the surface of separation, ripple-mark is easily formed.

Ripples were made in sand with a variety of fluids, but with olive oil it was found impossible. According to the views maintained in the present paper, the viscosity of oil is too great to permit the generation of the ripple-making vortices.

At p. 257, M. de Candolle writes—

"Chaque ride se termine à la partie supérieure par une crête composée des particules les plus légères. Tant que dure le balancement du liquide, les particules sont animées d'un mouvement pendulaire qui les transporte alternativement de part et d'autre de la crête. Aussi longtemps que l'amplitude de ce balancement est égale à celle qui a donné naissance aux rides, les particules mobiles parcourent à

* "On the Formation of Ripple-mark." Proc. Roy. Soc., April 20, 1882, vol. 34, p. 1.

† "Rides formées," &c. Archives des Sciences Physiques et Naturelles. Genève, No. 3, vol. ix, 15th March, 1883.

chaque demi-oscillation* toute la distance qui sépare l'une de l'autre deux crêtes consécutives. Ce va-et-vient des particules s'étend jusqu'à une certaine distance au-dessous du sommet de chaque crête, mais son amplitude va en diminuant de haut en bas, en raison du poids plus considérable des particules inférieures. Il en résulte que chacune de ces crêtes mobiles a l'apparence d'une lamelle qui oscille sur le sommet de la ride qu'elle termine et s'étire en même temps dans le sens du mouvement de l'eau, ce qui donne tout à fait l'apparence d'un corps visqueux.

"Lorsque l'amplitude du balancement du liquide diminue, il en est naturellement de même des excursions de ces lamelles, et si l'on vient à arrêter subitement le balancement, les particules composant les crêtes mobiles peuvent se déposer entre les rides où elles forment un système de rides secondaires plus minces, intercalées entre celles qui correspondent au maximum d'amplitude du balancement."

In this passage the dance of the particles is in the first place described as being from one side to the other of the ridge, and this, I believe, is the fact. This statement is, however, apparently contradicted by what follows, viz., that the dance is from crest to crest. I have very rarely seen the intercalated ripple-marks to which M. de Candolle refers, but I venture to think that his explanation is not sound, and that they are formed by the particles of sand which, in violent oscillation, have been caught up by the secondary or tree vortices, carried quite round and dropped at the root of the tree, when the oscillations of the water are dying out.

M. de Candolle arrives at the interesting conclusion that the wave-length of ripple-mark is independent of the nature of the oscillating fluid. His suggestion that cirrus clouds are ripple-marks between two aerial currents will be referred to below. The whole paper forms a valuable contribution, and should be read by those who are interested in the subject.

The last paper to which I shall refer is by M. Forel.† He has made extensive observations on ripple-mark, formed both naturally and artificially. He distinguishes between dunes formed by continuous currents either of air or water and ripple-marks formed by oscillation. His view accords with the experiments in § 1 above, but he has not apprehended the importance of the vortex in the lee of the

* In a letter M. de Candolle tells me that "demi-oscillation" should read "quart d'oscillation;" but I still do not see how the ambiguity pointed out below is removed by this correction.

† "Les Rides de Fond." Archives des Sciences Physiques et Naturelles. Genève, 15 Juillet, 1883. M. Forel quotes an article of mine in "Nature" as attributing the formation of ripple-mark to the action of currents in the sea. My statement was intended merely to imply that shallowness of water is favourable to the formation of ripple-mark. I had already made a great part of these experiments when that article was written in 1882.

dune, considering that region merely as slack water. I feel some doubt as to the view that a regular series of dunes may be formed by uniform current; at any rate, in my experiments the dunes were irregular, and had no definite wave-length.

His observations on the circumstances which govern the wave-length of ripple-mark are important. He finds that the factors which enter are the amplitude and period of oscillation of the water; and a third factor is the maximum velocity of the water, which he takes as identical with the ratio between the two others. If the water moves as a whole with a simple harmonic oscillation it is undoubtedly true that the ratio of amplitude to period is proportional to the maximum velocity, but the vortices quite disturb this relation. The maximum velocity of the water relatively to the bottom must depend upon the intensity of the vortices, and this depends upon the height of the ripple-mark.

M. Forel finds that the length and breadth of the vessel have no influence on wave-length, but that it diminishes with increasing depth of water. This he attributes to a diminution both of the period and of the amplitude of the oscillation of the water which is in contact with the bottom. The wave-length increases with the coarseness of the sand.

He remarks that when the ripple-mark is once made, the amplitude of oscillation is without influence on its wave-length. He draws attention to the two limiting velocities, one too great, and the other too small for the formation of ripples, for which values were found in the experiments of § 1.

M. Forel explains ripple-mark as the confluence of two dunes, formed alternately by the oscillating currents. This theory is undoubtedly correct, if somewhat incomplete.

The wave-length, he says, is the amplitude of oscillation of a grain of sand "librement transportée par l'eau." This expression requires further explanation; if it means the amplitude of oscillation of a particle of water at the bottom, when the oscillation is started, and before the ripples have risen, I am disposed to doubt it. It may mean the distance which an average grain of sand is transported when lying on the surface of other sand, under the like circumstances;* if so, ripple-marks formed with a thin layer of sand on a sheet of glass should have a longer wave-length than if the sand be thick; this I am also disposed to doubt. However this may be, M. Forel considers that the wave-length should vary directly as the amplitude of oscillation, directly as the velocity of the current, inversely as the density of the sand, and inversely as the size of the grains. Considering with M. Forel that the velocity of the current is

* I learn by a letter from M. Forel that this is his meaning.

the ratio of amplitude to period, we should have the wave-length for any one sand varying as the frequency multiplied by the square of the amplitude. The few fairly consistent experiments recorded in § 1, do not accord with this view, for it seemed that wave-length varied as v, which is proportional to frequency multiplied by amplitude. This I understand to accord with M. de Candolle's law. As to the law that the finer the sand the longer the wave-length, M. Forel justly observes that it is in contradiction with the fact that small ripples are formed by fine sand, and large ripples by coarse sand. But he endeavours to remove the apparent inconsistency by remarking in effect that the larger limiting velocity for fine sand is smaller than the smaller limiting velocity for coarse sand. There must undoubtedly be truth in this view, but I hesitate to accept it as the whole truth.

Noticing that, in the same sites in the Lake of Geneva, the ripples are always of the same length, he says, "de ces observations il semblerait résulter que l'intensité des vagues a bien peu d'influence sur la largeur des rides; que la nature du sol est le seul facteur important."

It appears to me that M. Forel's view as to the wave-length of ripple-mark cannot be accepted as final, but he has certainly thrown much light on the subject in his interesting paper.

The following considerations bear upon the laws of wave-length:—

It appeared that in the initial stages of ripple-making, the wave-length is at first only half as long as it becomes ultimately, and that when the layer of sand is thin, the wave-length always remains shorter than if it is thick. Hence if a little sand is dusted on to the oscillating sheet of glass, it is found that the wave-length of ripple is long in the middle of the patch of sand, and short near the margins. Thus the patch when ripple-marked presents such an appearance

Fig. 12.

as fig. 12. If the sand is thin, this appearance often persists however long the oscillation is maintained. This shows that wave-

length is a function of the height of the existing undulations; that is to say, not only of the amplitude of oscillation of the upper part of the vortices, but also of their intensity. On the parts of the plate where the sand is thick, a continual rearrangement of ripple-mark goes on; the wave-length extends by the excision of short patches of intercalated ripple-mark, and by general rearrangement. Finally the sand reaches an ultimate condition as regards wave-length, although rearrangement of ripple-mark still appears to go on for a long time. Then we find in this final condition most of the sand arranged with a certain fundamental wave-length, but where the sand is thin, patches remain with the octave or half wave-length.

It is not easy to understand precisely the mode in which the oscillation of the water over the undulating bottom gives rise to vortices, but there are familiar instances in which nearly the same kind of fluid motion must occur.

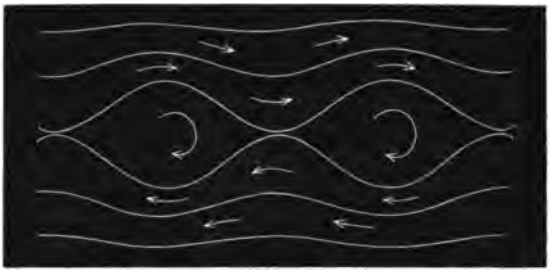
In the mode of boat propulsion called sculling, the sailor places an oar with a flat blade through a rowlock in the stern of the boat, and, keeping the handle high above the rowlock, waves the oar backwards and forwards with an alternate inclination of the blade in one direction and the other. This action generates a stream of water sternwards. The manner in which the blade meets the water is closely similar to that in which the slopes of two ripple-marks alternately meet the oscillating water; the sternward current in one case, and the upward current in the other are due to similar causes. We may feel confident that in sculling, a pair of vortices are formed with axes vertical, and that the dividing line between them is sinuous. The motion of a fish's tail gives rise to a similar rearward current in almost the same way. These instances may help us to realise the formation of the ripple-making vortices.

Lord Rayleigh has considered the problem involved in the oscillations of a layer of vortically moving fluid separating two uniform streams.* At the meeting of the British Association at Swansea in 1880, Sir William Thomson read a paper discussing Lord Rayleigh's problem.† He showed that, in a certain case in which the analytical solution leads to an infinite value, there are waves in the continuous streams in diametrically opposite phases, and that the vortical stratum consists of a series of oval vortices. Fig. 13 illustrates this mode of motion. The uniform current flowing over existing ripple-mark exhibits almost a realisation of this mode of motion, one of the streams of fluid being replaced by the sandy undulations. The same kind of motion must exist in air when a gust of wind blows a shallow puddle into standing ripples.

* "On the Stability or Instability of certain Fluid Motions." Proc. Lond. Math. Soc. (Feb. 12, 1880), vol. xi, p. 57.

† Nature, Nov. 11, 1880, pp. 45–6, and see correction on p. 70.

Fig. 13.



It seems probable that what is called a mackerel sky is an evidence of a closely similar mode of motion; and this agrees with M. de Candolle's suggestion that cirrus is aërial ripple-mark. The layer of transition between two currents of fluid is dynamically unstable, but if a series of vortices be interpolated, so as to form friction rollers as it were, it probably becomes stable. It is likely that in air a mode of motion would be set up by friction, which in frictionless fluid would be stable. If one of the currents of air be colder than the other, a precipitation of vapour will be caused in the vortices, and their shapes will be rendered evident by clouds.*

The direction of striation and velocity of translation of mackerel clouds require consideration according to this theory. If the velocity of the upper current be u, and of the lower current be $-u$, the interposed vortices have a velocity zero, and have their axes perpendicular to the velocity u. Hence if relatively to the earth the rectangular components of the upper current are $u+w$, v, and of the lower are $-u+w$, v, the component velocities of the vortices are w, v, and their axes are parallel to the component v.

Therefore the striations should be parallel to that direction in which the two currents have equal components, and the component velocity of the clouds parallel to the striations should be equal that of either current in the same direction. The resultant velocity of the clouds is clearly equal to a half of that of the two currents, and the component velocity of the striations perpendicular to themselves is the mean of the components of the two currents in the same direction.

[If one of the currents veers the axes of the vortices are shifted.

* I had suggested that the centrifugal force of the vortical rotation must produce rarefaction, fall of temperature, and precipitation of vapour. I have to thank Professor Stokes for pointing out that the fall of temperature would necessarily be so small as only to cause precipitation if the air were almost completely saturated. I think the fall of temperature at the centre of the vortex might be between a hundredth and a fiftieth of a degree Centigrade. [Sir W. Thomson tells me that he had given this vortical explanation before the B. A. in 1876. The volume, however, gives no abstract. He explains the formation of cloud as due to the upward motion in the vortices, and consequent rarefaction.—Jan. 4, 1884.]

The existing clouds will be furrowed obliquely, and the vortical stratum will be cut up into diamond-shaped spaces, determined by the intersection of the old and new vortex-axes. This would explain the patchwork arrangement commonly observed in mackerel sky. May not the lengths of the patches give a measure of the rate of veering of one of the currents ?—Jan. 4, 1884.]

The above account of the formation of ripple-marks shows it to be due to a complex arrangement of vortices. The difficulty of observation is considerable, and perhaps some of the conclusions may require modification. I hope that other experimenters may be induced to examine the question.

Lord Rayleigh has shown me a mathematical paper, as yet unpublished, in which he has considered the formation of aërial vortices over a vibrating plate. It seems possible that an application might be made of similar modes of approximation to the question of water oscillating over a corrugated bottom. Even a very rough solution would probably throw much light on the exact changes which the ripple-making vortices undergo, and any guidance from theory would much facilitate observation.

"On the Atomic Weight of Titanium." By T. E. Thorpe, F.R.S. Received November 7, 1883.

(Preliminary communication.)

The stoïchiometrical quantities which we ordinarily term atomic weights are not only the fundamental constants of chemical calculations; their relations as mere numbers are of the highest significance in connexion with our conceptions concerning the essential nature of matter.

The recent publications of Becker and Clarke in America, and of Lothar Meyer and Seubert in Germany, have served to demonstrate on how slight an experimental basis a large number—the greater proportion, it must be confessed—of the accepted values of these constants really depend.

A notable instance of this fact is seen in the case of titanium. The atomic weight of this element was determined by Rose in 1829, and by Pierre in 1847 with the following results:—

Rose..........	48·13 and 49·58,
Pierre........	50·25.

The commonly accepted value of titanium is that founded upon the experiments of Pierre; the atomic weight adopted by Mendelejeff in the series based upon his periodic law is 48, a number which finds

some experimental support from the observations of Rose. The direct evidence in favour of either value is, however, very slight, and a critical examination of the observations affords no ground for assuming that Rose's value is nearer the truth than that of Pierre. It is probable that the value 48 has been adopted in one or two modern text-books, mainly because it agrees best with the requirements of the periodic law.

I have attempted to deduce the atomic weight of titanium from observations made with the tetrachloride, the tetrabromide, and the dioxide. The present communication contains the results furnished by the analysis of the chloride. The work with this body affords three independent values for Ti based on the ratios—

I........	$TiCl_4 : 4Ag$
II........	$TiCl_4 : 4AgCl$
III........	$TiCl_4 : TiO_2$

Series I.—$TiCl_4 : 4Ag$.

Weighed quantities of the tetrachloride were decomposed by water in closed vessels, and the chlorine precipitated by silver after the method of Gay-Lussac as modified by Stas—

	Chloride taken.		Silver employed.		Ti (H = 1).
I........	2·43275	....	5·52797	....	48·06
II........	5·42332	....	12·32260	....	48·07
III........	3·59601	....	8·17461	....	47·99
IV........	3·31222	....	7·52721	....	48·05
V........	4·20093	....	9·54679	....	48·05
VI........	5·68888	....	12·92686	....	48·06
VII........	5·65346	....	12·85490	....	47·95
VIII........	4·08247	....	9·28305	....	47·94
	34·39004		78·16399		48·021

Series II.—$TiCl_4 : 4AgCl$.

Weighed portions of the tetrachloride after decomposition with water were treated with excess of silver nitrate, and the silver chloride separated by reverse filtration and weighed—

	Chloride taken.		Silver chloride.		Ti (H = 1).
IX........	3·31222	....	10·00235	....	47·99
X........	4·20093	....	12·68762	....	47·98
XI........	5·68888	....	17·17842	....	48·00
XII........	5·65346	....	17·06703	....	48·06
XIII........	4·08247	....	12·32442	....	48·06
	22·93796		69·25983		48·018

The total amount of the silver chloride thus obtained was 69·25983 grms.: it was formed from 53·13881 grms. of silver added in the preceding series. The weight of the silver chloride thus obtained from a known weight of silver and of titanium tetrachloride, not only affords an additional value for Ti, but it also serves as a rigorous check on the accuracy of the work, for if the ratio of Ag to AgCl is found to be the same as that obtained by the direct union of chlorine and silver, it at once disposes of the possibility of error due to the co-precipitation of titanic acid, and is a guarantee of the purity of the silver employed—

Now 52·13881 : 69·25983 = Ag : AgCl = 1 : 1·3284.

Stas found from seven experiments in which, in the aggregate, 969·3548 grms. of silver were found to give 1287·7420 grms. of silver chloride, that—

Ag : AgCl = 1 : 1·32845.

The two ratios, it will be seen, are almost identical.

Series III.—$TiCl_4 : TiO_2$.

The tetrachloride was decomposed by water, and the solution evaporated to dryness and strongly heated.

	Chloride taken.	Titanic oxide.	Ti (H = 1).
XIV	6·23398	2·62825	47·93
XV	8·96938	3·78335	48·00
XVI	10·19853	4·30128	47·95
XVII	6·56894	2·77011	47·96
XVIII	8·99981	3·79575	47·98
XIX	8·32885	3·51158	47·94
	49·29948	20·79032	47·970

All the experiments which were made, either for the estimation of the chlorine or the titanic oxide, are given with the exception of two; one of these was made upon a small quantity of material, and was considered as merely preliminary; the other miscarried and was not persevered with.

In order to enable an opinion to be formed as to the agreement among the several observations, I have calculated the value of Ti afforded by each experiment. But probably the most accurate values for the atomic weight would be deduced from the aggregate weights of the tetrachloride, silver, silver chloride, and titanic oxide, respectively. Inasmuch as it may be presumed that the employment of large quantities of material would tend to increase the accuracy of the result, by calculating the final values from the aggregate weights

instead of merely taking the means of the several observations, the influence of the larger quantities is directly felt.

Assuming with Lothar Meyer and Seubert that the most probable ratios of Ag, Cl, O, and H, are as follows—

$$\begin{aligned} Ag &= 6\cdot7456 \\ Cl &= 2\cdot21586 \\ O &= 1 \\ H &= 0\cdot06265 \end{aligned}$$

the various experiments afford the following values for Ti :—

I........	$TiCl_4 : 4Ag$	$=34\cdot39004$	$: 78\cdot16399$
		$= 1\cdot75989$	$: 4$
	$TiCl_4 : O$	$=11\cdot8715$	$: 1$
	$Ti : O$	$= 3\cdot0081$	$: 1$
	$Ti : H$	$=48\cdot014$	$: 1$
II........	$TiCl_4 : 4AgCl$	$=22\cdot93796$	$: 69\cdot25983$
		$= 1\cdot32475$	$: 4$
	$TiCl_4 : O$	$=11\cdot8716$	$: 1$
	$Ti : O$	$= 3\cdot0082$	$: 1$
	$Ti : H$	$=48\cdot016$	$: 1$
III........	$TiCl_4 : TiO_2$	$=49\cdot29948$	$: 20\cdot79032$
		$= 2\cdot37124$	$: 1$
	$Ti : O$	$= 3\cdot0053$	$: 1$
	$Ti : H$	$=47\cdot969$	$: 1$

On the assumption that these values have equal weight, the final value becomes—

I..........	48·014
II..........	48·016
III..........	47·969
	Ti=48·000

It would appear, therefore, from these observations, that titanium must be added to the increasing list of the elements whose atomic weights are simple multiples of that of hydrogen.

In these observations, which have occupied me many months, I have sought to eliminate such sources of error as were known to me. It is of course possible, in spite of the agreement between the several values, that the results may be affected by undetected and constant errors. Experience warns us that no determination of atomic weight, however well the individual observations may agree among themselves, can be considered wholly satisfactory if it depends upon a single reaction or is referred to a single relation. It is for this

reason that I have sought to extend my observations to other compounds of titanium, and to vary the nature of the reactions involved in the chemical process. Unfortunately it is found that comparatively few bodies containing titanium lend themselves to the purpose of atomic weight determination. I am, however, making observations with the tetrabromide, which, in some respects, is to be preferred to the tetrachloride, and the results furnished by its analysis will be given in a second communication, which will also contain details respecting the preparation of the substances used, the methods of weighing, the processes of manipulation, effect of errors, &c. With reference to the tetrabromide, I may here say that I find it can be very easily made by the action of hydrobromic acid gas upon the chloride, and that this proves to be a more convenient method of preparation than that by which it was first obtained by Duppa.

III. "On the Life History of the Dock Æcidium (*Æcidium rumicis*, Schlecth)." By CHARLES B. PLOWRIGHT. Communicated by W. THISELTON DYER, M.A., F.R.S. Received November 10, 1883.

This Æcidium, which is common in this country upon *Rumex hydrolapathum*, Huds., *obtusifolius*, Linn., *crispus*, Linn., and *conglomeratus*, Murray, was regarded by Fuckel* and Cooke† as being a condition of *Uromyces rumicis* (Schum.), is now stated by Winter‡ in his last work to be a condition of *Puccinia magnusiana*. During the present year I have conducted a series of cultures, in which the life history of this fungus has been carefully, if not laboriously, worked out, from which it appears that *Æcidium rumicis* bears the same relationship to *Puccinia phragmitis* (Schum.) (=*P. arundinacea*, D.C.) as *Æcidium berberidis*, Gmel., bears to *Puccinia graminis*, Perss.

History of the Subject.—Winter, in 1875,§ showed that those botanists who had associated this Æcidium with the *Uromyces rumicis*, simply because these two fungi occurred upon the same host plant, were wrong, and that the fungus in question was the æcidiospore of *Puccinia phragmitis*. Stahl, in 1876, repeated Winter's experiment, and confirmed it. Now it happens that there are two *Pucciniæ* common upon *Phragmitis communis*, the *P. phragmitis* (Schum.), and *P. magnusiana*, Körn.‖ In March, 1877, Schröter¶ placed the spores

* Fuckel, "Symbol. Mycol.," p. 64.

† Cooke, "British Uromyces," Grevillea, VII, p. 136.

‡ Winter, "Rabenhorst's Kryptogamen-Flora," 1881, p. 222.

§ Winter, "Hedwigia," 1875, vol. xiv, pp. 113–115.

‖ Körnicke, "Hedwigia," 1876, vol. xv, p. 179.

¶ Schröter Cohn, "Beitrage zur Biologie der Pflanzen," vol. iii, Heft I, pp. 65–66.

of both these *Pucciniæ* upon *Rumex hydrolapathum* (the species Winter originally experimented with), and found that the Æcidium was only produced from *P. magnusiana*. Winter,* in the "Kryptogamen-Flora," now in course of publication, accepts Schröter's statement, and gives as the æcidiospores of *Puccinia magnusiana*, not only the Æcidium on *Rumex hydrolapathum*, but also on *R. crispus*, *conglomeratus*, *obtusifolius*, and *acetosa*, and adds a note to the effect that the Æcidium upon *Rheum officinale* has probably the same life history.

Personal Investigations.—In 1882 I performed a number of experimental cultures for the purpose of personally observing the life history of the heterœcismal uredines generally. For instance, upon two occasions, a handful of reed leaves and stems were laid upon healthy plants of *Rumex conglomeratus;* after a time these plants became affected with *Æcidium rumicis*, and I naturally concluded that this had arisen from the spores of *Puccinia magnusiana* which I had observed upon the reeds employed. Afterwards, upon three separate occasions, when I had become more expert in performing these experiments, I applied the spores of *P. magnusiana* to other plants of *Rumex conglomeratus* and once to *R. obtusifolius*, but without producing any Æcidium. Subsequent research has rendered clear that on the reeds used in my first two experiments, both *Pucciniæ* were present. Being desirous of finding, if possible, the æcidiospores of *P. phragmitis*, if this plant possessed any, I this year placed its spores upon various *Ranunculi* (*R. repens* and *Ficaria*), but without any result. It then occurred to me that Winter might be in error in affiliating all the dock æcidia to *P. magnusiana*, and that perchance on some of them the æcidiospores of *P. phragmitis* might occur. This presumption was favoured by the fact that the æcidia we have been in the habit in this country of lumping together as *Æcidium ranunculacearum* include two species which, although resembling each other very closely in appearance, have distinct life histories. I, therefore, on 16th May (1883), placed spores† of *Puccinia phragmitis* upon *Rumex crispus*, and in due time obtained an abundant crop of *Æcidium rumicis*. On 16th June, when the Æcidium was perfectly mature, I placed some of its spores upon two plants of *Phragmitis communis*, where it produced, first the uredo (distinguished from the uredo of *P. magnusiana* by the absence of paraphyses), and later the perfect *Puccinia phragmitis*. Subsequently plants of *Rumex hydrolapathum*, *obtusifolius*, *conglomeratus*, and *Rheum officinale*, were successfully infected with *Puccinia phragmitis* spores; whereas the infection of other specimens of these plants with *Puccinia magnusiana* was in all

* Winter, "Rabenhorst's Kryptogamen-Flora," 1881, p. 222.

† The word spore is here employed in the sense that De Bary employs it, namely, to indicate the body produced by the promycelium of the teleutospore. I have elsewhere spoken of these spores as "promycelium spores."

cases without result. Both *Pucciniæ* were also applied to *Rumex acetosa*, but with no result. These experiments are appended in a tabular form, showing the plant infected, the spores used, the date of infection, and the date the resulting spores were first observed. The numbers refer to my own note-book.

No. of expt.	Plant infected.	Spore used for infection.	Date. Of infection.	Date. Of 1st result.
			1882.	
18	Rumex conglomeratus..	Puccinia magnusiana ?..	6 May	3 June
32	,, ,, ..	,, ,, ?..	15 May	3 June
			1883.	
137	Ranunculus repens	Puccinia phragmitis ...	18 May	
138	,, ,, ...	,, ,, ...	18 May	
142	,, ficaria	,, ,, ...	20 May	
			1882.	
34	Rumex conglomeratus..	Puccinia magnusiana...	18 May	
70	,, ,, ..	,, ,, ...	15 June	
81	,, ,, ..	,, ,, ...	15 June	
			1883.	
167	,, ,, ..	,, ,, ...	5 June	
			1882.	
72	,, obtusifolius	,, ,, ...	15 June	
			1883.	
169	,, ,, ...	,, ,, ...	5 June	
168	,, crispus	,, ,, ...	5 June	
171	,, hydrolapathum.	,, ,, ...	7 June	
205	,, ,, .	,, ,, ...	28 June	
184	Rheum officinale.......	,, ,, ..	12 June	
187	,, ,, ...	,, ,, ...	13 June	
			1883.	
140	Rumex crispus	Puccinia phragmitis....	16 May	27 May
178	,, conglomeratus..	,, ,, ...	8 June	22 June
179	,, obtusifolius	,, ,, ...	8 June	19 June
180	,, ,, ...	,, ,, ...	8 June	19 June
181	,, hydrolapathum.	,, ,, ...	8 June	19 June
182	Rheum officinale.......	,, ,, ...	8 June	19 June
183	,, ,, ...	,, ,, ...	8 June	19 June
148	Phragmitis communis ..	Æcidium rumicis	27 May	4 June
166	,, ,, ..	,, ,, ...	3 June	12 June
172	,, ,, ..	,, ,, ...	7 June	
188	,, ,, ..	,, ,, ...	16 June	10 July
189	,, ,, ..	,, ,, ...	16 June	10 July
208	,, ,, ..	,, ,, ...	2 July	20 July
209	,, ,, ..	,, ,, ...	2 July	30 July
139	Rumex acetosa	Puccinia magnusiana...	17 May	
179	,, ,, ...	,, ,, ...	5 June	
206	,, ,, ...	,, ,, ...	28 June	
177	,, ,, ...	,, phragmitis ...	8 June	

Conclusions:—

1. That the æcidiospores of *Puccinia phragmitis* occur on *Rumex hydrolapathum, crispus, obtusifolius, conglomeratus*, and *Rheum officinale*.

2. That the æcidiospores of *Puccinia magnusiana* have yet to be discovered if the *Puccinia* has any, which it probably has.

3. That the Æcidium on *Rumex acetosa* is not connected with either *Puccinia phragmitis* or *magnusiana*.

IV. "Some Relations of Heat to Voltaic and Thermo-Electric Action of Metals in Electrolytes." By G. Gore, LL.D., F.R.S. Received November 14, 1883.

(Abstract.)

The author states that the object of this research was "to ascertain the thermo-electric action of a series of metals in particular liquids, and to examine the relations of the thermo-electric to the chemico-electric behaviour of metals in electrolytes, and to ordinary chemical corrosion, and the source of the voltaic current."

With this object, the thermo-electric tension series, also the chemico-electric tension series, at 60° and 160° F. of thirteen metals in solutions of twenty-two different salts, alkalies, and acids, were experimentally found, and are shown in two tables. The same thermo- and chemico-electric series were also determined in solutions of different strengths of the same substances, and are also exhibited in two tables; and the effects of difference of strength of liquid upon the orders of those series are inferred from the results.

Several other tables contain series of determinations of the electric potentials, in volts, of numerous thermo-electric, and also of chemico-electric couples of the same series of metals in the weak solutions and in the strong ones, and show the influence of strength of liquid upon thermo-electric and chemico-electric potential. The measurements of potential were made by means of a new form of thermopile (see "Proc. Birm. Phil. Soc.," vol. iv, Part 1).

The influence of temperature upon the chemico-electric potential of those metals in the series of weak solutions, and also in the strong ones, at 60° and 160° F., was also determined, and the results are given in several tables.

The "voltaic reversal points," or temperatures at which the two metals of a voltaic pair balanced each other, and produced no current, in fifty-four different cases discovered during the research, were also determined and are given. The metals and solutions employed were the same as those used in the previous series of experiments.

The potentials of voltaic currents with the same series of metals in the weak solutions at 60° F. were also determined in seventy-five instances, in each case in thermo-electric terms of one of the same metals in the same liquid; and the number of degrees of difference of temperature of the particular thermo-electric couple required to balance the corresponding voltaic couple in each instance is given in a table.

A series of experiments was also made to determine whether the difference in potential, caused by heating a voltaic couple, is completely accounted for by the electromotive forces produced by the same rise of temperature of the same metals separately in the same liquids, and the results are shown in a table.

With considerable difficulty, a series of about 120 determinations were also made of rates of ordinary chemical corrosion of the same metals in solutions of the same substances at 60° F., and a second series of about seventy-five determinations with the same metals and liquids at 160° F. were also made, and the results are given in two tables.

These and other results are contained in a series of twenty tables; and by comparing the contents of these various tables with each other, and also by means of additional experiments, a number of general conclusions were obtained. The following are the conclusions, and the chief results arrived at by these means.

It is shown that when metals in liquids were heated, they were more frequently rendered positive than negative in the proportion of about 2·8 to 1; and that while the proportion in weak solutions was about 2·29 to 1, in strong ones it was about 3·27 to 1; and this accords with their thermo-electric behaviour as metals alone. The thermo-electric order of metals in liquids was, with nearly every solution, whether weak or strong, widely different from the thermo-electric order of the same metals alone. A conclusion previously arrived at was also confirmed, viz., that the liquids in which the hot metal was thermo-electro-positive in the largest proportion of cases, were those containing highly electro-positive bases, such as the alkali metals. The thermo-electric effect of *gradually* heating a metal in a liquid was sometimes different from that of *suddenly* heating it, and was occasionally attended by a reversal of the current.

Degree of strength of liquid greatly affected the thermo-electric order of the metals. Increase of strength usually and considerably increased the thermo-electric potential of metals thermo-electro-negative in liquids, and somewhat decreased that of those positive in liquids. The electric potential of metals thermo-electro-positive in weak liquids was usually about 3·87 times, and in strong ones 1·87 times as great as of those which were negative. The potential of the strongest thermo-electric couple, viz., that of aluminium in weak

solution of sodic diphosphate, was 0·66 volt for 100 F. degrees difference of temperature, or about 100 times that of a bismuth and antimony couple.

Heating one of the metals, either the positive or negative, of a voltaic couple usually increased the electric difference, making most metals more positive and some more negative, whilst heating the second one also usually neutralised to a large extent the effect of heating the first one. The electrical effect of heating a voltaic couple was nearly wholly composed of the united effects of heating each of the two metals separately, but was not, however, exactly the same, because whilst in the former case the metals were dissimilar and heated to the same temperature, in the latter they were similar but heated to different temperatures:—also, when heating a voltaic pair, the heat was applied to two metals, both of which were previously electro-polar by contact with each other as well as by contact with the liquid, but when heating one junction of a metal and liquid thermo-couple, the metal had not been previously rendered electro-polar by contact with a different one, and was therefore in a somewhat different electric state. When a voltaic combination in which the positive metal was thermo-negative and the negative one was thermo-positive was heated, the electric potential of the couple diminished, notwithstanding that the internal resistance was decreased.

Magnesium in particular, also zinc and cadmium, were greatly depressed in electro-motive force in electrolytes by elevation of temperature. Reversals of position of the two metals of a voltaic couple in the tension series by rise of temperature, were usually due to one of the two metals increasing in electro-motive force faster than the other, and in many cases to one metal increasing and the other decreasing in electro-motive force, but only in a few cases was it a result of simultaneous but unequal diminution of potential of the two metals. With eighteen different voltaic couples, by rise of temperature from 60° to 160° F., the electro-motive force in twelve cases was increased, and in six decreased; and the average proportions of increase for the eighteen instances was 0·1 volt for the 100 F. degrees of elevation.

A great difference in chemical composition of the liquid was attended by a great change in the order of the volta tension series; and the differences of such order in two similar liquids, such as solutions of hydric chloride and potassic chloride, were much greater than in those produced in either of those liquids by a difference of 100° F. of temperature. Difference of strength of solution, like difference of composition or of temperature, altered the order of such series with nearly every liquid, and the amount of such alteration produced by an increase of four or five times in the strength of the liquid was rather less than that caused by a difference of 100 F. degrees in temperature.

Whilst also variation of strength of liquid caused only a moderate amount of change of order in the volta tension series, it produced more than three times that amount of change in the thermo-electric tension series. The usual effect of increasing the strength of the liquid upon volta-electro-motive force was to considerably increase it, but its effect upon thermo-electro-motive force was to largely decrease it. The degree of potential of a metal and liquid thermo-couple was not always exactly the same at the same temperatures during a rise as during a fall of temperature; this is analogous to the variation of melting and fusing points of bodies under such conditions, and also to that of supersaturation of a liquid by a salt, and is probably due to some hindrance to change of molecular movement.

The rate of ordinary chemical corrosion of each metal varied in every different liquid; in each solution also it differed with every different metal. The most chemically positive metals were usually the most quickly corroded, and the corrosion of each metal was usually the fastest with the most acid solutions. The rate of corrosion was dependent both upon the nature of the metal and upon that of the liquid, and was limited by the most feebly active of the two—usually the electrolyte. The order of rate of corrosion of metals also differed in every different liquid. The more dissimilar the chemical characters of two liquids, the more diverse usually was the order of rapidity of corrosion of a series of metals in them. The order of rate of simple corrosion in any of the liquids examined differed largely from that of degree of chemico-electric, and still more from that of thermo-electro-motive force. Corrosion was not the cause of pure thermo-electric action of metals in liquids.

Out of fifty-eight cases of rise of temperature the rate of ordinary corrosion was increased in every instance except one, and that was only a feeble exception; the increase from 60° to 160° F. with different metals was extremely variable, and was from 1·5 to 321·6 times. Whether a metal increased or decreased in thermo-electro or chemico-electro-motive force by being heated, it increased in rapidity of corrosion. The proportions in which the most corroded metal was also the most thermo-electro-positive one was 65·87 per cent. in liquids at 60° F., and 69·12 in the same at 160° F.; and the proportion in which it was the most chemico-electro-positive at 60 F. was 84·44 per cent., and at 160° F. 80·77 per cent. The proportion of cases therefore in which the most chemico-electro-negative metal was the most corroded one increased from 15·56 to 19·23 per cent. by a rise of temperature of 100 F. degrees. Comparison of these proportions shows that corrosion usually influenced in a greater degree chemico-electric than thermo-electric actions of metals in liquids. Not only was the relative number of cases in which the volta negative was the most corroded, increased by rise of temperature, but also the average relative loss by

corrosion of the negative to that of the positive one was increased from 3·11 to 6·32.

The theory most consistent with all the various results and conclusions is a kinetic one. That metals and electrolytes are throughout their mass in a state of molecular vibration. That the molecules of these substances being frictionless bodies in a frictionless medium, and their motion not being dissipated by conduction or radiation, they continue incessantly in motion until some cause arises to prevent them. That each metal (or electrolyte) when unequally heated has, to a certain extent, an unlike class of motions in its differently heated parts, and behaves in those parts somewhat like two metals (or electrolytes), and those unlike motions are enabled through the intermediate conducting portion of the substance to render these parts electro-polar. That every different metal and electrolyte has a different class of motions, and in consequence of this they also by contact alone with each other at the same temperature become electro-polar. The molecular motion of each different substance also increases at a different rate by rise of temperature.

This theory is equally in agreement with the chemico-electric results. In accordance with it, when in the case of a metal and an electrolyte the two classes of motions are sufficiently unlike, chemical corrosion of the metal by the liquid takes place, and the voltaic current originated by inherent molecular motion under the condition of contact, is maintained by the portion of motion lost by the metal and liquid during the act of uniting together. Corrosion, therefore, is an effect of molecular motion, and is one of the modes by which that motion is converted into electric current.

In accordance with this theory, if we take a thermo-electric pair consisting of a non-corrodible metal and an electrolyte (the two being already electro-polar by mutual contact), and heat one of their points of contact, the molecular motion of the heated end of each substance at the junction is altered; and as thermo-electric energy in such combinations usually increases by rise of temperature, the metal and liquid, each singly, usually becomes more electro-polar. In such a case the unequally heated metal behaves to some extent like two metals, and the unequally heated liquid like two liquids, and so the thermo-electric pair is like a feeble chemico-electric one of two metals in two liquids, but without corrosion of either metal. If the metal and liquid are each when alone thermo-electro-positive, and if when in contact the metal increases in positive condition faster than the liquid by being heated, the latter appears thermo-electro-negative, but if less rapidly than the liquid, the metal appears thermo-electro-negative.

As also the proportion of instances is small in which metals that are positive in the ordinary thermo-electric series of metals only,

become negative in the metal and liquid ones (viz., only 73 out of 286 in weak solutions, and 48 out of the same number in strong ones), we may conclude that the metals more frequently than the liquids have the greatest thermo-electric influence, and also that the relative largeness of the number of instances of thermo-electro-positive metals in the series of metals and liquids, as in the series of metals only, is partly a consequence of the circumstance, that rise of temperature usually makes substances, metals in particular, electro-positive. These statements are also consistent with the view that the elementary substances lose a portion of their molecular activity when they unite to form acids or salts, and that electrolytes therefore have usually a less degree of molecular motion than the metals of which they are composed.

The current from a thermo-couple of metal and liquid, therefore, may be viewed as a united result of difference of molecular motion, first of the two junctions, and second of the two heated (or cooled) substances; and in all cases, both of thermo- and chemico-electric action, the immediate true cause of the current is the original molecular vibrations of the substances, whilst contact is only a static permitting condition. Also, that whilst in the case of thermo-electric action, the sustaining cause is molecular motion supplied by an external source of heat, in the case of chemico-electric action it is the motion lost by the metal and liquid, when chemically uniting together. The direction of the current in thermo-electric cases appears to depend upon which of the two substances composing a junction increases in molecular activity the fastest by rise of temperature, or decreases the most rapidly by cooling.

In a separate paper "On some Relations of Chemical Corrosion to Voltaic Currents," the author has investigated the amounts of external voltaic current produced by the corrosion of known weights of various metals at atmospheric temperature.

V. "On a 'Rennet' Ferment contained in the Seeds of Withania coagulans." By Sheridan Lea, M.A., Trinity College, Cambridge. Communicated by Professor M. Foster, Sec. R.S. Received November 20, 1883.

The Report of the Royal Gardens at Kew for 1881 contains abstracts of correspondence, in which it was pointed out that, in order to introduce a cheese-making industry in India, some vegetable substitute must be found for the ordinary animal rennet, since cheese made with the latter is unsaleable among the natives. In response to the above "Surgeon-Major Aitchison brought to the notice of the autho-

rities at Kew that the fruit of *Puneeria** *coagulans*, a shrub common in Afghanistan and Northern India, possesses the properties of coagulating milk;" and experiments showed that an aqueous extract of the seed-capsules of the above plant does somewhat rapidly coagulate milk.

I was recently requested to make some experiments on the seeds of Withania to determine whether they contain a definite ferment with the properties of ordinary rennet, and the applicability of such a ferment to cheese-making purposes.

The material supplied to me consisted of an agglomerated dry mass of seed-capsules and fragments of the stalks of the plant. When crushed in a mortar the whole crumbled down into a coarse powder, in which the seeds were for the most part liberated from the capsules. I picked out the larger pieces of stalk, sifted out the finer particles, chiefly earth and fragments of the capsules, and then by a further sifting I separated the seeds from the other larger particles. The seeds appeared to be each enveloped in a coating of resinous material, presumably the dried juice of the capsules in which they had ripened.

Taking equal weights of the seeds, I extracted them for twenty-four hours with equal volumes of (i) water, (ii) 5 per cent. sodic chloride, (iii) 2 per cent. hydrochloric acid, (iv) 3 per cent. sodic carbonate. Equal volumes of each of the above were added in an acid, alkaline, and neutral condition, to equal volumes of milk, and heated in a water-bath at 38° C. The milk was rapidly coagulated by the salt and sodic carbonate extracts, much less rapidly by the other two; of the four, the salt extract was far the most rapid in its action. All subsequent experiments have shown that a 5 per cent. solution of sodic chloride is the most efficient in the extraction of the active principle from the seeds.

There is no doubt that the substance which possesses the coagulating power is a ferment closely resembling animal rennet.

I. A portion of the 5 per cent. sodic chloride extract loses its activity if boiled for a minute or two.

II. The active principle is soluble in glycerine, and can be extracted from the seeds by this means; the extract possesses strong coagulating powers even in small amounts.

III. Alcohol precipitates the ferment body from its solutions; and the precipitate, after washing with alcohol, may be dissolved up again without having lost its coagulating powers.

IV. The active principle of the seeds will cause the coagulation of milk when present in very small quantities, the addition of more of the ferment simply increasing the rapidity of the change.

* The genus Puneeria is now reduced by botanists to Withania.

V. The coagulation is not due to the formation of acid by the ferment. If some of the active extract be made neutral or alkaline and added to neutral milk, a normal clot is formed, and the reaction of the clot remains neutral or faintly alkaline.

VI. The clot formed by the action of the ferment is a true clot, resembling in appearance and properties that formed by animal rennet, and is not a mere precipitate.

Having thus determined the presence of a rennet ferment in the seeds, I endeavoured to prepare an active extract, which should be applicable for cheese-making purposes. All the extracts of the seeds are of a deep brown colour, and it appeared, therefore, in the first place, desirable to obtain less highly coloured, if not colourless, solutions, which should still be active. In this I have so far failed. The precipitate caused by alcohol carries down the chief part of the colouring-matter also, so that on being subsequently redissolved the solution is nearly as highly coloured as before the precipitation. The colour can be removed by using animal charcoal, but the ferment is at the same time destroyed. If all excess of charcoal is avoided and the solution is filtered at once, the filtrate is largely decolorised, but contains only traces of the ferment. Animal rennet is similarly removed by filtration through charcoal. The colour can be removed by the addition of very finely-powdered kaolin in a dry state, but, as before, the ferment activity is thereby destroyed. The same holds good of animal rennet. The colouring-matter is scarcely soluble in either ether or alcohol, so that no advantage is gained by a preliminary treatment with these before extraction with the salt solution. I have also endeavoured to get rid of the colour by treating the seeds as rapidly as possible with successive quantities of water before making the final extract. By using a centrifugal machine I was able to wash the seeds six or seven times with large volumes of water without their being exposed for any considerable time to the action of the water. Each portion of water was highly coloured and the seeds were thus freed from adherent colouring-matter. But, apart from the fact that some, though not much, ferment is thus lost, no special advantage is obtained, since the seeds are themselves coloured, and even after prolonged treatment with water the final extract is always of a dark brown colour.

In order to obviate the disadvantages of this colouring-matter, if disadvantage it is, I have found it best to prepare very concentrated active extracts of the purified seeds, so that it should only be necessary to add a very small quantity of the extract in order to coagulate the milk and obtain a colourless curd. This I have done by grinding the dry seeds very finely in a mill and extracting them for twenty-four hours with such a volume of 5 per cent. sodic chloride solution that the mass is still fluid after the absorption of water by the fragments

of the seeds as they swell up. From this mass the fluid part may be readily separated by using a centrifugal machine (such as is used in sugar refining), and it can then be easily filtered through filter-paper; without the centrifugal machine the separation of the fluid from the residue of the seeds is tedious and imperfect, 40 grms. of the seeds treated as above with 150 cub. centims. of 5 per cent. sodic chloride solution gave an extract of which 0·25 cub. centim. clotted 20 cub. centims. of milk in twenty-five minutes, and 0·1 cub. centim. clotted a similar portion of milk in one hour. When added in these proportions the curd formed is quite white. The presence of the colouring-matter is, however, perhaps on the whole unimportant, since even if a larger quantity of the ferment extract is added in order to obtain a very rapid coagulation the colouring-matter is obtained chiefly in the whey, the curd being white.*

The question of preparing an extract which should be capable of being kept for a considerable time is perhaps of importance. Ordinary commercial rennet usually contains a large amount of sodic chloride and some alcohol. One specimen I analysed contained 19 per cent. of common salt, and 4 per cent. of alcohol. I have, therefore, added to the 5 per cent. chloride extract mentioned above, enough salt to raise the percentage of this to 15 per cent., and also alcohol up to 4 per cent. The activity of the extract is not appreciably altered by this, and such a preparation corresponds very closely in activity with a commercial solution of animal rennet with which I compared it. The possibility of making extracts which may be expected to keep is thus indicated, but of course time alone will show whether the activity of the ferment is impaired to any important extent by such keeping.

I may add in conclusion that I have coagulated a considerable volume of milk with an extract such as I have described, and prepared a cheese from the curds. I have also given a portion of the extract to a professional cheese-maker who has used it as a substitute for animal rennet in the preparation of a cheese. The product thus obtained, and the statements of the person who has made the experiment for me, lead me to suppose that extracts of the seeds of Withania can be used as an adequate and successful substitute for animal rennet.

* It is extremely probable that some stage in the growth or ripening of the seeds of Withania might be found at which the development of colouring-matter is slight, while at the same time the ferment is present in considerable quantity.

November 30, 1883.

ANNIVERSARY MEETING.

THE PRESIDENT in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts during the past year, including a balance of £1,940 14*s.* 3*d.* carried from the preceding year, amount to £20,873 19*s.* 4*d.*; and that the total expenditure in the same period, including purchase of ground rents, amounts to £21,258 10*s.* 3*d.*, leaving a balance of £384 10*s.* 11*d.* due to the Bankers, and £7 4*s.* 3*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Boase, Henry S., M.D.
Canterbury, His Grace Archibald Campbell Tait, Lord Archbishop of, D.D.
Challis, Rev. James, F.R.A.S.
Eastwick, Edward Backhouse, C.B.
Howard, John Eliot, F.L.S.
Hudson, Robert, F.G.S.
Jessel, The Right Hon. Sir George, Knt., Master of the Rolls.
Marcet, Francis.
Pearson, Sir Edwin, Knt., M.A.
Potter, Edmund.
Sabine, General Sir Edward, R.A., K.C.B.
Scott, Henry Young Darracott, Major-Gen., R.E., C.B.
Siemens, Sir William, D.C.L., LL.D.
Smith, Henry John Stephen, M.A., D.C.L.
Spottiswoode, William — PRESIDENT—M.A., D.C.L.
Stebbing, Rev. Henry, D.D.
Talbot de Malahide, James, Lord, M.A.
Varley, Cromwell Fleetwood, M.I.C.E.
Walker, Charles Vincent, F.R.A.S.
Watson, Sir Thomas, Bart., M.D.
Young, James, LL.D.

On the Foreign List.

Bischoff, Theodor Ludwig Wilhelm von.
Plateau, Joseph Antoine Ferdinand.

Re-admitted.

Armstrong, Henry Edward, Ph.D.

Fellows elected since the last Anniversary.

Aitchison, James Edward T., Surgeon-Major, M.D.
Browne, James Crichton, M.D., LL.D.
Chamberlain, Right Hon. Joseph.
Dobson, George Edw., Surgeon-Major, M.A., M.B.
Duncan, James Matthews, A.M., M.D.
Fitzgerald, Prof. George Francis, M.A.
Flight, Walter, D.Sc.
Frost, Rev. Percival, M.A.
Gill, David, LL.D.
Groves, Charles Edward, F.C.S.
Grubb, Howard, F.R.A.S.
Langley, John Newport, M.A.
Reinold, Arnold William, M.A.
Trimen, Roland, F.L.S., F.Z.S.
Venn, John, M.A.
Walker, John James, M.A.

The President then addressed the Society as follows:—

GENTLEMEN,

It will be as much in consonance with your feelings as it is with my own, that the first sentences of this address should give utterance to our sense of the calamity which befell us during the recess.

On the 27th of June, our honoured and loved President, William Spottiswoode, fell a victim to that cruel malady, typhoid fever, which is at once the scourge and the reproach of modern civilisation; and we were bereaved of a chief of whom all those who had the highest interests of this Society at heart, hoped that he would continue, for many a year, to discharge the responsible and laborious duties of his office with that broad intelligence, that faithful diligence, that inexhaustible patience and courtesy, which were so characteristic of the man.

Every one of the Fellows of the Society, in whose hearing I speak, knows that these are no words of conventional eulogy, as of a customary epitaph. But, it is only those of us who worked with our late President in the Council, or as officers of the Society, who are in a position fully to appreciate his singular capacity for the transaction of business with clear judgment and rapid decision; and yet with the most conscientious consideration of the views of those with whom he was associated.

And, I may add, that it is only those who enjoyed Mr. Spottiswoode's intimate friendship, as it was my privilege to do for some quarter of a century, who can know how much was lost, when there vanished from among us that rare personality, so commingled of delicate sensitiveness with marvellous self-control, of rigid principle with genial tolerance, of energetic practical activity with untiring benevolence, that it always seemed to me the embodiment of that exquisite ideal of a true gentleman which Geoffry Chaucer drew five hundred years ago:—

". . . He lovede chyvalrye,
Trouthe and honour, fredom and curtesie.
. . . . . .
And though that he was worthy he was wys,
And of his port as meke as is a mayde.
He never yit no vilonye ne sayde
In al his lyf unto no maner wight.
He was a verray perfight gentil knight."

It is not for me to pass any judgment upon Mr. Spottiswoode's scientific labours; but I have the best authority for saying, that having occupied himself with many branches of mathematics, more especially with the higher algebra, including the theory of determinants, with the general calculus of symbols, and with the application of analysis to geometry and mechanics, he did excellent and durable work in all; and that, in virtue of his sound and wide culture, his deep penetration, and the singular elegance with which he habitually treated all his subjects, he occupied a place in the front rank of English mathematicians.

The interment in Westminster Abbey of one who, though compelled to devote a large share of his time to business, was a born man of science, and had won himself so high a place among mathematicians, was doubtless grateful to us as men of science; it could not but be satisfactory to us, as Fellows of the Royal Society, that, on the rare occasion of the death of our President in office, the general public should show its sympathy with our bereavement; yet, as men, I think it is good to regard those solemn and pathetic obsequies as the tribute which even our busy, careless, cynical, modern world spontaneously pays to such worth and wisdom, to such large humanity and unspotted purity, as were manifested in the "very perfect gentle knight" who so well represented the chivalry of science.

The total number of Fellows deceased during the past year amounts to twenty-one; a large inroad upon our ranks in mere numbers, an exceptionally severe mortality if we consider the scientific rank of many names in the death-roll. Almost at the same time with Mr. Spottiswoode's untimely death, we lost, at the ripe old age of ninety-five, a very distinguished Fellow and former President of this Society, Sir Edward Sabine. It is said that the average age of Fellows of the Royal Society is greater than that of any body of men in Europe; and it is certainly a remarkable fact that one who so long presided over us in this generation, should, as a man of thirty years, have been the contemporary of Sir Joseph Banks, who became our President more than a century ago. And nothing can give a more striking exemplification of the gigantic progress of physical science in modern times than the fact that the discovery of oxygen by Priestley, and that of the composition of water by Cavendish, fall

within the period of Sir Joseph Banks' presidency, while Black's work was but a score years earlier. We are as it were but two Presidents off the budding of modern chemistry, as of many another stately growth of the tree of natural knowledge.

Sir Edward Sabine's long services to this Society, first as Treasurer and then as President, deserve more than a passing allusion; but for a due appreciation of them, no less than of his great labours in terrestrial magnetism, I must refer you to our obituary notices.

By the unexpected death of Professor Henry John Stephen Smith the University of Oxford lost one of the most distinguished, as he was one of the most influential, among those who have guided its destinies during this generation, and a capacity of the first order, not yet weakened by the touch of time, has disappeared from the ranks of the foremost mathematicians of Europe.

As Chairman of the Meteorological Committee, Professor Smith rendered invaluable services to that body; and we have all a grateful recollection of the readiness with which his knowledge and sagacity were brought to our aid in Council and in Committee.

For the rest, I dare add nothing to that which has been said of him by our late President in that just and loving appreciation of his friend, which is now touched with a sadder gravity and a deeper pathos.

It is difficult to say of Professor Smith whether he was more remarkable as a man of affairs, of society, of letters, or of science; but it is certain that the scientific facet of his brilliant intelligence was altogether directed towards those intelligible forms which people the most ethereal regions of abstract knowledge. In Sir William Siemens, who, but the other day, was suddenly snatched from among us, we had a no less marked example of vast energy, large scientific acquirements, and intellectual powers of a high order, no less completely devoted, in the main, to the application of science to industry.

I believe I am expressing the opinion of those most competent to judge, when I say that Sir William Siemens had no superior in fertility and ingenuity of invention; that hardly any living man so thoroughly combined an extensive knowledge of scientific principles with the power of applying them in a commercially successful manner; and that the value of his numerous inventions must be measured, not merely by the extent to which they have increased the wealth and convenience of mankind, but by the favourable reaction on the progress of pure science which they, like all such inventions, have exerted, and will continually exert.

Time compels me to be but brief in alluding to the remainder of our long list of deaths. But I may not omit to mention that we have lost a distinguished mathematician in Professor Challis; in Mr. James Young, a chemist whose skilful application of theory to practice established a new industry; in Mr. Cromwell Varley, an

ingenious inventor; in Lord Talbot de Malahide, a warm friend of science and a zealous promoter of archæological research; in Mr. Walker, an eminent engineer; in John Elliot Howard, a distinguished quinologist; and, in the Rev. Dr. Stebbing, an accomplished and amiable man of letters, who for very many years filled the honourable, but not very onerous, office, of Chaplain to the Society.

And it would ill become us, intimately connected as this Society always has been, and I hope always will be, with the sciences upon which medicine bases itself, to leave unnoticed the decease of the very type of a philosophical physician, the venerable Sir Thomas Watson.

Two well-known names have disappeared from among those of the eminent men who are enrolled upon our foreign list; the eminent physicist Plateau, and the no less distinguished anatomist and embryologist Bischoff.

I now beg leave to bring under your notice a brief general review of the work of the Society during the past year.

The papers printed in the "Transactions" for 1882 and 1883 will occupy two volumes, of which three parts, containing 1038 4to. pages and 52 plates, have already been published. Two parts more, to complete 1883, will shortly be published.

The "Proceedings," which steadily increase in size from year to year, amount during the past year to 780 8vo. pages, with four plates and numerous engravings.

You are aware that nothing is printed in the "Proceedings" or in the "Transactions" except by the authority of the Council, which, in the latter case, calls in the assistance of at least two carefully-selected and independent referees, by whose advice it is in practice, though not necessarily, guided. I am inclined to think that Fellows of this Society who do not happen to have served on the Council, are little aware of the amount, or of the value, of the conscientious labour which is thus performed for the Society by gentlemen whose names do not appear in our records. And I trust I may be forgiven for stepping beyond precedent so far as to offer our thanks for work, which is always troublesome and often ungrateful; but, without which, the contributions to our pages would not maintain the high average of excellence which they possess.

Among the points of importance, by reason of their novelty or general significance, which have been laid before us, much interest attaches to the result brought out in Professor Osborne Reynolds' "Experimental Investigation of the circumstances which determine whether the motion of water shall be direct or sinuous, and of the law of resistance in parallel channels"; which shows that when the conditions of dynamical similarity are satisfied, two systems, involving

fluids treated as viscous, may be compared (as regards their effects) even when the motions are unstable; and that if any one of the two systems is in the critical state separating stability from instability, so will be the other.

Last December, Dr. Huggins presented a note on "A Method of Photographing the Solar Corona without an Eclipse," which had so far proved successful, under the unfavourable circumstances in which he had put it in practice, as to lead to the hope that, under better conditions of atmosphere and elevation, the corona might be photographed, from day to day, with so much accuracy as to preserve a clear record of the changes which it undergoes. And, as the photographs taken during the eclipse at Caroline Island show a condition of the corona, intermediate between those exhibited by Dr. Huggins' photographs at periods antecedent and subsequent to the Caroline Island observations, there is reason to believe that this hope is well based, and that a new and powerful method of investigation has been placed in the hands of students of solar physics.

Lord Rayleigh and his sister-in-law, Mrs. Sidgwick, have made a very elaborate determination of the relation between the ohm and the British Association standard of electrical resistance.

With respect to those branches of knowledge on which I may venture to offer an opinion of my own, I may say that, though our records show much useful and praiseworthy work in biological science, the only event which appears to me to call for special remark is the opening of an attack upon a problem of very great interest, one which, in fact, goes to the root of the question of the fundamental unity of the two great embodiments of life—plants and animals.

The well-known phenomena presented by many plants, such as the sensitive plant and the sun-dew, our knowledge of which was so vastly extended by Darwin, abundantly prove that the property of irritability, that is, the reaction of a living part, by change of form, upon the application of a stimulus to that part, or to some other part in living continuity with it, is not confined to animals.

But, in animals, the connexion of the part irritated with that which changes its form is always effected by a continuity of more or less modified protoplasmic substance, and reaction takes place only so long as that continuity is unimpaired; while, hitherto, the protoplasmic cell-bodies of plants have appeared to be isolated from one another by the non-protoplasmic cell-walls in which they are inclosed.

It is as if, in the one case, there was a continuous bond of conducting substance between the point of irritation and the point of contraction; while, in the other, there was a chain of pellets of protoplasmic substance, each inclosed in a coat of a different nature.

Now, Mr. Gardiner, in his paper "On the continuity of the Protoplasm through the Walls of Vegetable Cells," brings forward evidence,

based chiefly upon the careful use of special reagents, that, in the sensitive cushions of certain plants and in other situations, the vegetable cell-wall is pierced by minute apertures, and that these are traversed by threads of protoplasm, which connect the cell-body of each cell with those of its neighbour, and thus establish, as in animals, a continuity of protoplasmic substance between different parts. Other observers are working at the same subject, and we may hope that, before long, great light will be thrown upon many hitherto puzzling questions in vegetable physiology.

The Committee of the Royal Society, in the hands of which the Lords of the Treasury have placed the administration of the funds devoted to the publication of the work of the "Challenger" expedition, report that, under the careful and vigorous direction of Mr. Murray, this great undertaking is making rapid progress.

Mr. Murray informs me that thirty-eight reports have, up to this time, been published, forming eight large quarto volumes, with 4195 pages of letter-press, 488 lithographic plates and other illustrations. Thirty-four of these memoirs are on zoological, four on physical subjects. Nine reports are now nearly all in type, and some of them partly printed off. These will be published within three months, and will form three zoological volumes, with 230 plates and many wood-cuts, and one physical volume, with many diagrams and maps; this latter volume will contain the report on the composition of ocean water, the specific gravity and temperature observations.

A considerable part of the general narrative of the cruise is now in type, and nearly all the illustrations are prepared. The narrative will extend to two volumes, and it is expected they will be ready for issue in May or June, 1884.

The work connected with the remaining forty-two special reports is, in most instances, progressing satisfactorily. Portions of the manuscript for three of the larger memoirs have been received and put in type, and the manuscript of many others is in a forward state. For these memoirs, 386 lithographic plates have been printed off and delivered to the binders; 404 others are now on stone, and the drawings for many more are being prepared. It is estimated that the whole work connected with the Report will be completed in the summer of 1887.

In his Address, last year, the President gave the Society a full account of the changes which had taken place in the administration of the Government Fund—technically termed a grant in aid of this Society—though, as you are aware, the Royal Society, while willingly accepting the burden and the responsibility of administrator of the aid granted by the State to science, is, in no sense, pecuniarily benefited by the grant.

A somewhat novel and extremely useful employment has been given to part of the fund by deciding to defray the expenses of adequately-skilled persons who have undertaken to visit distant countries for the purpose of investigating certain interesting biological questions on the spot, and of procuring and transmitting to observers at home, specimens prepared and preserved by those refined modern methods which can be satisfactorily carried out only by persons who are well versed in the practice of such methods.

Mr. Adam Sedgwick has thus been enabled to proceed to the Cape of Good Hope for the purpose of completing our knowledge of the singular genus *Peripatus*, so well studied by Professor Moseley and afterwards by our lamented Fellow, Balfour; and Mr. Caldwell, similarly aided, is now in Australia, devoting himself to the elucidation of the embryology of the marsupial quadrupeds of that region, a subject of which, at present, we know little more that was made known in the Transactions of this Society half a century ago by Professor Owen.

It certainly was high time that British science should deal with a problem of the profoundest zoological interest, the materials for the solution of which abound in, and are at the same time almost confined to, those territories of the Greater Britain, which lie on the other side of the globe.

Many years ago, the late Mr. Leonard Horner communicated to the Society the results of a series of borings which he had caused to be made in the upper part of the delta of the Nile, with a view of ascertaining the antiquity of the civilisation of Egypt. Since that time, Figari Bey, an Italian geologist in the service of the Egyptian Government, made and published the results of a large series of borings effected in different parts of the delta, but his work is hardly on a level with the requirements of modern science.

It has been thought advisable, therefore, to take advantage of the presence of our troops in Egypt, in order to carry out a series of borings across the middle of the delta, in the full expectation that such borings, if made with proper care and carried down to the solid rock, will afford information of the most important character, and will throw a new light upon the natural and civil history of this unique country. I am glad to say that the representations which the President and Council made to the War Office on this subject were most favourably received, and that instructions were at once sent to the officer commanding the Engineers to undertake the operations which they recommended. I trust that, before long, information will reach us which will be of no less interest to the archæologist than to the geologist.

While I am speaking of Egypt, I may, perhaps, be permitted

to express a regret that the admirable energy of the Government, in taking measures to make the recent advances of medical science available during the late outbreak of cholera in that country, was not extended beyond the purely practical side of the matter, or, perhaps, not so far as the practical side in the proper sense; for, until we know something about the causes of that terrible disease, our measures for prevention and for cure will be alike leaps in the dark.

Those who have looked into the literature of cholera may, perhaps, be disposed to think that a new search after its cause will add but another to the innumerable wild hypotheses which have been set afloat on that topic; and yet, devastating epidemics, like the pebrine of the silkworm, so similar in their fatality and their apparently capricious spread, that careful investigators have not hesitated to institute a detailed comparison of the phenomena of this disease with those of cholera, have been proved by Pasteur to be the work of microscopic organisms; and hardly less fatal epidemics, such as splenic fever, have been traced to similar agencies. In both these cases, knowledge of the causes and of the conditions which limit the operation of the causes, have led to the invention of effectual methods of cure. And it is assuredly, in the present state of science, something more than a permissible hypothesis, that the cause of cholera may be an organic living *materies morbi*, and that the discovery of the proper curative and prophylactic measures will follow upon the determination of the nature and conditions of existence of these organisms.

If this reasoning is just, it is certainly to be regretted that the opportunity of the outbreak of cholera in Egypt was not utilised for the purposes of scientific investigation into the cause of the epidemic. There are able, zealous, and courageous young pathologists in this country who would have been willing enough to undertake the labour and the risk; and it seems a pity that England should leave to Germany and to France an enterprise which requires no less daring than Arctic or African exploration; but which, if successful, would be of a thousand times more value to mankind than the most complete knowledge of the barren ice wastes of the pole or of the sweltering barbarism of the equator.

It may be said that inquiries into the causation of cholera have been for some years conducted in India by the Government without yielding any very definite result. But this is, perhaps, rather an argument in favour of, than against, setting fresh minds to work upon the problem.

In December last year the President received from the Lords of the Treasury a letter, addressed to their Lordships by the Lords of the Committee of the Privy Council on Education, recommending to the favourable consideration of the Treasury a memorial from the Solar Physics Committee, suggesting the organisation of an expedition

for the purpose of making observations during the solar eclipse of the 5th of May, 1883; and the President was requested to communicate his views upon the subject to the Treasury.

After careful consideration, the President and Council reported in favour of the projected expedition; but they added that they did so on condition of its being possible to find some one, whose position in the scientific world would command the confidence of the public, to take charge of the expedition. Unfortunately, for one reason or another, none of the men of science who fulfilled this condition were able to go; and, at the meeting of Council of the 18th of January, the projected expedition was abandoned. The President was, however, requested to place himself in communication with the American authorities, and to ascertain from them whether a photographer and assistant could be allowed to accompany their expedition to Caroline Island. On doing so, he at once received an invitation for two observers; who were accordingly sent out, their expenses being defrayed, partly, by a contribution from the Government Grant and, partly, by a special sum of £500 provided by the Treasury.

I am indebted to Mr. Lockyer for the following list of photographs taken by the observers:—

"1. Six good photographs of the corona, exposures varying from two to sixty seconds, giving coronal detail from near the limb to end of streamers. That the limit of the corona has been photographed is shown by the manner in which the light of the sky has impressed itself on the plate.

"2. Three large photographs showing the details of the corona close to the limb.

"3. Good photographs of the spectrum of the corona, showing a great number of coronal lines and very faint Fraunhoferic lines.

"4. Photographs taken on a moving plate in integrating spectroscope, from one minute before to one and a half minute after totality, showing the most prominent lines of the reversion spectrum. These lines belong mainly to hydrogen.

"5. Photographs taken with first-order grating, before, during, and after totality. These show H and K, near the limb, throughout the whole of totality.

"6. Photographs taken with a dense prism spectroscope before, during, and after totality. These photographs also give some of the prominent lines of the reversion spectrum.

"7. Two photographs taken in the prismatic camera on plates sensitive to ultra-red rays. Results comparatively indifferent on account of the absence of prominences."

The arrangements made for obtaining a series of Circumpolar Observations in Meteorology and Magnetism were fully described in the Presidential Address of last year. I am glad to be able to

report that the English party, under Captain Dawson, has successfully achieved its mission and has returned to this country. Captain Dawson speaks very gratefully of the efficient assistance which he received from the Canadian authorities and from the Hudson Bay Company.

The responsibility for the transaction of the ordinary work of the Society rests with the Council and the officers, of whom the President is only one, and I may be allowed to say by no means the most important, the heaviest part of the burden of the executive resting upon the Treasurer and the Secretaries. But your President is, in virtue of his office, a member of two public bodies whose functions in relation to science are of great importance; and I follow the excellent precedent set by my predecessor in considering it my duty to acquaint the Fellows of the Society with any occurrence, bearing on the interests of science, which has come under my cognizance, as a Trustee of the British Museum and as a Member of the Council and Executive Committee of the City and Guilds Institute.

In the first-named capacity, I am glad to be able to announce that the transference of the vast zoological, botanical, geological, and mineralogical collections from Bloomsbury to the New Natural History Museum is now accomplished; and that it has been effected, to the great credit of all concerned, with no greater mishap than the fracture of a bottle or two.

The advantages which will accrue to zoologists, botanists, and mineralogists from the re-arrangement of this vast assemblage of the objects of their studies, in such a manner as to be accessible to every investigator, cannot be over-estimated. The Natural History Museum at South Kensington is, in fact, a library of the works of nature which corresponds in value, in extent, and in the purposes to which it should be applied, to the vast library of the works of men which remains at Bloomsbury.

In making this collection of use to the world of science by the publication of complete catalogues of its contents, and of systematic monographs upon particular groups; and to the nation at large, by the composition of guide books calculated to afford the ordinary visitor an insight into the plan of the mighty maze of nature, the officers in charge of the Natural History collections have before them a task, the due performance of which, whatever their abilities, or their number, or their industry, will tax their energies to the utmost. It is in this way that, in the discharge of their proper duties, they may render services of the highest value alike to pure science and to the diffusion of knowledge among the people, out of whose resources the great institution to which they belong is supported. And I trust that no mistaken view of the functions of the officers of the Museum,

which no more embrace oral instruction in science than those of the officers of the Library comprehend oral instruction in literature, may lead to the imposition of duties, foreign to their proper business, upon the already overburdened staff of keepers and their assistants.

In Francis Bacon's apocalypse of science, the "New Atlantis," the Father of Solomon's House—he, whose countenance was "as if he pitied men,"—declares that the end of that foundation is "the knowledge of causes and secret motions of things, and the enlarging of the bounds of human empire to the effecting of all things possible."

I think that the Chancellor would have acknowledged the New Natural History Museum to be a goodly wing of such a House, devoted to the former of the objects which he mentions; but, it may be, that his practical mind, looking always to fruit, and caring for light chiefly as something essential to fruit-bearing, would have been even better satisfied with another building hard by, which has been devoted to the encouragement of those applications of science through which human empire is directly extended, by the well-directed munificence of the City and Guilds of London.

This building, destined for a central institution in which ample provision shall be made for thorough and practical training in so much of the principles and the methods of the physical sciences as is needful for those who aspire to take part in the development of arts and manufactures, has been completed at a cost of more than £70,000, while £20,000 has yet to be spent upon fittings and appliances, and the working expenses, if the scheme is to be fully developed, cannot be estimated at less than £10,000 a year.

Having already been called upon to take an active part in the deliberations of the committees charged with the carrying out of this great work, I think I am justified in expressing the hope, and indeed the confident expectation, that, before long, this new Technical College will be in full activity; and that, for the first time in our history, there will be called into existence an institution in which, without leaving this country, masters, managers, and foremen of works will be enabled to obtain thorough instruction not only in scientific theory, but in the essential principles of practice; and a machinery will be created, by which the poorest working lad in a manufacturing town, if he have ability and perseverance, may be brought within reach of the best technical education that is to be had.

There can be no doubt that the founders of the Royal Society had prominently before their minds the intention of promoting the useful arts and sciences "that so (in the language of the draft of the preamble to the first charter, which is said to have been drawn up by Sir Christopher Wren) by laying in a stock as it were of several arts and methods of industry, the whole body [of the nation] may be supplied by a mutual commerce of each other's peculiar faculties, and,

consequently, that the various miseries and trials of this frail life may be, by as many various expedients ready at hand, remedied or alleviated, and wealth and plenty diffused in just proportion to every one's industry, that is, to every one's deserts." It was the wish of King Charles the Second that all patents for inventions should be examined by the Royal Society; and, so late as the reign of George the Second, the Society actually performed this duty. The steam-engine itself may be said to have made its *débût* before the Royal Society, when Savery exhibited his working model to the Fellows in 1699.

But the subsequent history of natural knowledge has shown that, as in the moral world, those who seek happiness through well-doing are less likely to obtain that reward than those who try to do well without thinking what may come of it; so, in the world of science, those whose vision is fixed on useful ends are often left poor and bare, while those who strive only after the advancement of knowledge, scatter riches along their path, for the whole world to pick up. The Royal Society has chosen the latter course, and I trust it may never swerve from it. But I think that our warmest sympathy is due to the efforts of those who translate the language of the philosopher into that of the workshop; and by thus ameliorating "the miseries and toils of this frail life," and "diffusing wealth and plenty," are executing that part of the first design of this Society, with which we, as a body, have long ceased to occupy ourselves.

It was not as your President, but as one of the Special Commissioners appointed by the Government, that I had some slight share in another considerable undertaking directed towards the improvement of industry. But the future of the fisheries is so closely connected with the advancement of certain branches of zöological science, that I may be permitted to advert to the great success of the International Fisheries Exhibition; and to express my belief that, in accordance with the intimation contained in the speech of H.R.H. the Prince of Wales at the closing of the Exhibition, there will grow out of it an organisation which will provide for the application of science to the improvement of the fisheries.

In conclusion, Gentlemen, I think that it is proper on my own behalf, as it is certainly due to you, that I should advert to the exceptional circumstances which have brought about my present occupation of the Presidential office.

The eleventh section of the sixth chapter of the Statutes provides for the occurrence of a vacancy in the Chair, whether by death or by resignation, as follows:—

"Upon any vacancy in the President's place occurring in the intervals of the anniversary elections, the Treasurer, or in his absence

one of the Secretaries, shall cause the Council to be summoned for the election of a new President, and the Council meeting thereupon in the usual place, or any eleven or more of them, shall proceed to the said election, and not separate until the major part of them shall have agreed upon a new President."

This statute is substantially, and, to a great extent, verbally, identical with the twelfth section of the seventh chapter of the original statutes of 1663.

Before the present year, five occasions had arisen on which it became necessary to put the provisions of the statute into effect.

Sir Isaac Newton died while President in 1727; the Earl of Morton in 1768; Mr. West in 1772; and Sir Joseph Banks in 1820; while Sir Humphry Davy resigned in 1827. On each of these occasions, a new President was at once appointed by the Council, endowed with all the privileges and powers of the office; and, like every other officer, however appointed, he vacated his office on the 30th November following, when the Fellows sometimes elected him for the succeeding year, and sometimes did not.

These precedents were strictly followed on the present occasion. A Council had been summoned, in ordinary course of business, for the 28th of June; but, as the President died on the 27th, the meeting was deferred until the following Thursday, when it was supposed the interment would have taken place. In consequence of the delay inseparable from a public ceremony, however, it so happened that the funeral did not take place until noon of the 5th of July; and I have known few sadder scenes than the gathering of the Council, fresh from the unclosed grave of their President, for the performance of the duty, imposed upon them by the Statutes, of choosing his successor from their own number, before they should separate.

The Council did me the great honour of selecting me for the office; and now, on this next following St. Andrew's Day, my tenure, like that of the Treasurer and Secretaries, lapses; and it is for the Fellows of the Society to say who shall be their officers until the next Anniversary Meeting.

Having served several years, in another capacity, with three out of four of my present colleagues, and having every reason to believe that the Fellows of the Society, at large, see good reason to set the same high value upon the services of all of them as I do, I do not find myself able to imagine that you will fail to desire that those services shall be continued; but I have not the least difficulty in conceiving that the Fellows of the Society may think many of their number better fitted for the eminent place of the President than myself.

I should be extremely ungrateful to my colleagues of the Council, who have again honoured me by presenting me for election by the Fellows, if I were to let fall even a hint of the extent to which I

share that opinion; but I think it may be permitted me to say that, should you think fit to give effect to it, there is no one who will more cheerfully acquiesce in your decision than I shall.

To a man like myself, who neither possesses, nor seeks, any other distinction than that of having done his best to advance knowledge and to uphold the worth and the authority of Science against all comers, the Presidency of this Society is the highest dignity which he can attain, whatever else may befall him.

But, Gentlemen, as men of science, you know better than I can tell you, that there are things of more worth than distinction. I am within measurable distance of the end of my career; and I have long looked forward to the time when I should be able to escape from the distractions and perturbations of the multitudinous affairs in which I have been so long entangled, to that student life from which the Fates have driven me, but to which I trust they may, for a little space, permit me to return.

So that I am sure you will neither misunderstand me, nor mislike my directness of speech, when I say that, if it please you to believe that the interests of Science and of the Royal Society will be advanced by maintaining me in the very distinguished position which I at present occupy, I will do my best to justify your confidence; but if, as may well be, you think that some other Fellow of the Society will serve these interests better, I shall, with a light heart, transfer to him the honourable burden, which I have already borne long enough to know its weight.

I now proceed to the presentation of the medals which have this year been awarded by the Council.

The President then proceeded to the presentation of the Medals :—

The number, the variety, and the importance of Sir William Thomson's contributions to mathematical and experimental physics are matters of common knowledge, and the Fellows of the Society will be more gratified than surprised to hear that the Council have this year awarded him the Copley Medal, the highest honour which it is in their power to bestow.

Sir William Thomson has taken a foremost place among those to whom the remarkable development of the theory of thermodynamics and of electricity, in the last forty years, is due; his share in the experimental treatment of these subjects has been no less considerable; while his constructive ability in applying science to practice is manifested by the number of instruments, bearing his name, which are at present in use in the physical laboratory and in the telegraph office.

Moreover, in propounding his views on the universal dissipation

of energy and on vortex motion and molecular vortices, Sir William Thomson has propounded conceptions which belong to the *prima philosophia* of physical science, and will assuredly lead the physicist of the future to attempt once more to grapple with those problems concerning the ultimate construction of the material world, which Descartes and Leibnitz attempted to solve, but which have been ignored by most of their successors.

One Royal Medal has been awarded to Dr. T. Archer Hirst, F.R.S., for his investigations in pure geometry; and, more particularly, for his researches into the correlation of two planes and into the complexes generated by them.

The other Royal Medal has been awarded to Dr. J. S. Burdon Sanderson, F.R.S., for the eminent services which he has rendered to physiology and pathology; and, especially, for his researches on the electrical phenomena exhibited by plants, and for his investigations into the relation of minute organisms to disease.

In making this award, the Council desire not merely to recognise the merit of Dr. Burdon Sanderson's researches, especially those which demonstrate the analogy between the electrical changes which take place in the contractile tissues of plants and those which occur in the like tissues of animals; but to mark their sense of the important influence which Dr. Sanderson has exerted upon the study of physiology and pathology in this country.

The Davy Medal has this year been again awarded in duplicate to M. Marcellin Berthelot, Member of the Institute of France, and Foreign Member of the Royal Society, and Professor Julius Thomsen, of Copenhagen.

The thermo-chemical researches of Berthelot and Thomsen have extended over many years, and have involved an immense amount of work, partly in the application of established methods to new cases, partly in devising new methods and applying them to cases in which the older methods were not applicable. Chemists had identified a vast variety of substances, and had determined the exact composition of nearly all of them, but of the forces which held together the elements of each compound they knew but little. It was known that certain elements combine with one another with great evolution of heat-forming products in which they are firmly united; while other elements combine but feebly, and with little evolution of heat. But the materials for forming any general theory of the forces of chemical combination were but scanty and imperfect.

The labours of Messrs. Berthelot and Thomsen have done much towards supplying that want, and they will be of the utmost value for the advancement of chemical science.

The Statutes relating to the election of Council and Officers were then read, and Professor Flower and Mr. Stainton having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year :—

President.—Professor Thomas Henry Huxley, LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.

Captain W. de Wiveleslie Abney, R.E.; Professor W. Grylls Adams, M.A., F.C.P.S.; The Duke of Argyll, K.T., D.C.L.; John Gilbert Baker, F.L.S.; Thomas Lauder Brunton, M.D., Sc.D.; William Henry M. Christie, Astron. Royal; Warren De La Rue, M.A., D.C.L.; Sir Frederick J. O. Evans, K.C.B.; Professor George Carey Foster, B.A.; Francis Galton, M.A., F.G.S.; James Whitbread Lee Glaisher, M.A.; Sir William Withey Gull, Bart., M.D.; Hugo Müller, Ph.D.; Professor Joseph Prestwich, M.A., F.G.S.; Professor Osborne Reynolds, M.A.; Osbert Salvin, M.A., F.L.S.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1882 ..	5	46	222	206	49	528
Since Elected ..				+ 1	+ 15	16
Since Re-admitted				+ 1		+ 1
Since Deceased ..		— 2	— 13	— 8		— 23
Nov. 30, 1883 ..	5	44	209	200	64	522

Statement of Receipts and Expenditure from November 24, 1882, to November 27, 1883.

				£	s.	d.
Balance at Bank, December, 1882	£1,929	0	0	1,940	14	8
" on hand	11	14	8			
Annual Contributions, 204 at £4	816	0	0	1,008	0	0
" 64 at £3	192	0	0			
Admission Fees				10	0	0
Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				214	0	0
Rents				122	10	0
Ground ditto				355	15	11
Dividends (exclusive of Trust Funds)				1,635	19	8
" on Jodrell Fund				151	15	8
Interest on Mortgage Loan				583	2	6
Sale of Transactions and Proceedings				868	8	10
Deposit withdrawn from Bankers				18,217	13	7
Interest on ditto	£51	13	9	156	1	6
	104	7	9			
				£20,204	1	6

	£	s.	d.	£	s.	d.
Salaries and Wages				1,081	17	0
The Library Catalogue				431	14	9
Catalogue of Scientific Papers				101	5	6
Books for the Library				248	13	3
Printing Transactions, Parts 2, 3, and 4, 1882, and Part 1, 1883, and Separate Copies to Authors and Publisher	702	6	8	2,496	4	11
Ditto Proceedings, Nos. 215–221	342	1	11			
Ditto Miscellaneous	73	18	6			
Paper for Transactions and Proceedings	364	4	0			
Binding ditto	91	1	0			
Advertising ditto	1	19	9			
Engraving and Lithography	920	13	6			
Soirée and Reception Expenses				84	5	10
Coal, Lighting, &c.	81	16	7	742	6	5
Office Expenses	16	1	6			
House Expenses	194	17	4			
Tea Expenses	22	15	2			
Fire Insurance	41	15	0			
Taxes	42	0	10			
Advertising	38	5	6			
Postage, Parcels, and Petty Charges	32	11	2			
Miscellaneous Expenses, including alterations to Telescope	272	3	4			
Law Charges				59	19	4
Law Charges and Expenses, Acton Estate				195	7	1
Mablethorpe Schools, Donation				2	2	0
Advanced to Eclipse Expedition				356	10	7
Transit of Venus, Expenses				0	5	0
Purchase of Ground Rents				14,850	1	4
				£20,650	18	0

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	416	8	6			
Rumford Fund Dividends	67	16	2			
Wintringham Fund "	35	0	6	609	17	10
Copley Medal Fund "	58	9	2			
Davy Medal Fund "	32	3	6			
Bankers' account overdrawn				384	10	11
				£21,258	10	3

	£	s.	d.	£	s.	d.
Donation Fund	305	0	0			
Davy Medal Fund	62	12	0			
Rumford do. do.	136	3	10			
Wintringham Fund	35	5	0	600	13	0
Copley Medal Fund	54	13	5			
Bakerian Lecture	4	0	0			
Croonian Lecture	2	18	9			
Balances on hand, Catalogue Account				7	4	3
				£21,258	10	3

JOHN EVANS,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.
" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s*. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £500 16*s*. 10*d*. in 1882–83).
£21,000 { £14,952 12*s*. 3*d*. Reduced 3 per Cent. Annuities. / £6,047 7*s*. 9*d*. " " " Handley Fund. }
£15,000 Mortgage Loan, 4 per Cent. " " "
£19,314 0*s*. 2*d*. Consolidated Bank Annuities, { being £15,861 19*s*. 1*d*. in name of the R. S., and £3,452 1*s*. 1*d*. in Chancery, arising from sale of the Coleman Street Estate. }
£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock—Bakerian and Copley Medal Fund.
£11,511 6*s*. New Threes { £6,328 11*s*. 2*d*. Scientific Relief Fund. / 5,182 14*s*. 10*d*. Jodrell Fund. }
£667 5*s*. 6*d*. India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.
£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.
£100 Metropolitan $3\frac{1}{2}$ per Cent. Stock.—Scientific Relief Fund.
£1,800 " " —Fee Reduction Fund.
£7,000 London and North Western Railway 4 per Cent. Debentures. } " "
Two Hundred Shares in the Whitworth Land Company, Limited. } " "
£5,000 Madras Railway Guaranteed 5 °/₀ Stock.
£5,000 North Eastern Railway 4 °/₀ Stock.
£5,000 London and North Western Consolidated 4 °/₀ Preference Stock.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct; and we find that a Balance of £384 10*s*. 11*d*. is due to the Bankers.

W. GRYLLS ADAMS.
T. LAUDER BRUNTON.
FRANCIS GALTON.
G. G. STOKES.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct; and we find that a Balance of £384 10*s*. 11*d*. is due to the Bankers.

J. T. BOILEAU.
J. R. HIND.
WILLIAM POLE.
JOHN RAE.

Trust Funds. 1883.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,328	11	2
Metropolitan 3½ Consols	100	0	0
	£6,428	11	2

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance	193	18	3	By Grants	130	0	0
„ Dividends	188	16	0	„ Balance	252	14	3
	£382	14	3		£382	14	3

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

The Trevelyan Bequest.

£1,396 Great Northern Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	671	11	3	By Grants	305	0	0
„ Dividends	239	7	0	„ Balance	782	19	9
„ Transferred from Handley Fund	177	1	6				
	£1,087	19	9		£1,087	19	9

Rumford Fund.

£2,822 19*s.* Consols.

	£	s.	d.		£	s.	d.
To Balance	136	3	10	By Medals	59	14	4
„ Dividends, 1883	67	16	2	„ Capt. Abney	76	9	6
				„ Balance	67	16	2
	£204	0	0		£204	0	0

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* Consols.
£403 9*s.* 8*d.* New 2½ per Cent.

	£	s.	d.		£	s.	d.
To Balance	102	12	6	By Gold Medal	4	13	5
„ Dividends	9	16	3	„ Prof. Cayley, Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	48	12	11	„ Bakerian Lecture	4	0	0
				„ Balance	102	8	3
	£161	1	8		£161	1	8

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1882	23	7	0	By Payment to Foreign Secretary	23	7	0

Wintringham Fund.

£1,200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1882	35	5	0	By Payment to Foundling Hospital, 1883	35	5	0
„ Dividends, 1883	35	0	6	„ Balance	35	0	6
	£70	5	6		£70	5	6

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1882	2	18	9	By Croonian Lecture	2	18	9
„ One-fifth of Rent of Estate at Lambeth Hill, receivable from the College of Physicians	2	18	9	„ Balance	2	18	9
	£5	17	6		£5	17	6

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	138	19	0	By Gold Medals	62	12	0
„ Dividends	32	3	6	„ Balance	108	10	6
	£171	2	6		£171	2	6

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£200 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	162	18	3	By Payments to Kew Committee	493	11	1
„ Dividends	499	7	10	„ Balance	168	15	0
	£662	6	1		£662	6	1

Handley Fund.

£6,047 7*s.* 9*d.* Reduced.

	£	s.	d.		£	s.	d.
Dividends, 1883	177	1	6	By transferred to Donation Fund	177	1	6

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1883	151	15	3	By transferred to Royal Society General Account	151	15	3

Fee Reduction Fund.

£1,800 Metropolitan Consols 3½ per Cent.

£7,000 London and North Western Railway 4 per Cent. Debentures.

Two Hundred Shares in the Whitworth Land Company, Limited.

	£	s.	d.		£	s.	d.
To Balance (1882)	298	17	5	By purchase of £250 Metrop. 3½ per Cent.	263	15	0
„ Dividends	526	5	8	„ transferred to Royal Society General Account (1883)	214	0	0
				„ Balance	347	8	1
	£825	3	1		£825	3	1

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from Vol. XXXIV, p. 340).

1882–83.

	£
J. N. Lockyer, for Spectroscopic Researches in connexion with the Spectrum of the Sun, for the purchase of apparatus and materials	100
J. L. E. Dreyer, for the expense of printing a "Second Armagh Catalogue" of about 3,000 stars deduced from meridian observations made in the years 1868–82	80
F. D. Brown, for Researches in the measurement, or rather estimation of the attraction which Molecules exert upon other contiguous similar or dissimilar Molecules	150
Dr. A. Macfarlane, for a quantitative investigation of the laws and values of the Electricity produced by the friction of different substances	50
Dr. G. J. Hinde, for Researches into the Structure and Classification of the British Fossil Sponges	50
Rev. J. F. Blake, for aid in preparing and publishing a work on British Fossil Cephalopoda	100
W. Heape, for investigation of the early stages of Marsupial embryology, with a view to the elucidation of problems of mammalian development	50
A. G. Bourne, for a research into the development of the Hag-fish, inclusive of the expense of specimens	50
Dr. Matthew Hay, for researches into the action of various cathartics, as bearing on their relative therapeutic values; and into the digestion of cane-sugar	50
Dr. Sanderson, for continuation of photographic investigations into the electrical phenomena which are associated with the functional activity of Contractile Protoplasm	50
A. Scott, for continuation of Researches into the Behaviour of the Salts of Rubidium and Cæsium and other Substances at High Temperature	50
Dr. H. E. Armstrong, for assistance in carrying on Researches into Naphthaline Derivatives	100
Prof. M. F. Heddle, for continuation of a Research connected with the Scientific Mineralogy and Geognosy of Scotland	300
Carried forward	£1,180

Brought forward	£1,1[illegible]
Prof. T. Rupert Jones, for further examination and determination of the Fossil Entomostraca (Ostracoda)	100
W. Gardiner, for an investigation on the continuity of protoplasm of vegetable cells, with a view to throw light on the phenomena of sensitive plants and plant-irritability in general	100
H. Tomlinson, for continuation of Investigations on the influence of Stress and Strain on the action of physical forces	250
V. H. Veley, for an Investigation of the rate of a Chemical change in which a gas is evolved from a homogeneous liquid ..	50
Prof. T. G. Macgregor, for Experiments on the Electromotive Force of Polarisation	35
G. F. Rodwell, for continuation of Research on the Coefficients of Contraction and Expansion of certain Bodies at Temperatures exceeding 100° C.	75
W. Baily, for the cost of construction of an Integrating Anemometer and Recording Arrangement. (See "Phil. Mag.," Sept., 1882)	50
Prof. P. G. Tait, for the construction, on a large scale, of the new form of Spectroscope described by him in "Nature" (August 19, 1880); (2) for very exact Measurements of the Rotatory Polarisation in different specimens of Quartz; (3) for the Improvement of our Methods of Studying the Spectra of Auroras, &c., by means of handy Instruments	50
R. T. Glazebrook, for obviating certain defects in Instruments used for Polarised Light	15
G. J. Symons, for the expense of a Computer in reducing Observations of Temperature made at considerable Heights ..	25
A. Frazer, for continuation of Experiments on Wind-pressure and additional Apparatus	25
E. A. Schäfer, for Wages of an Assistant in Investigations into the Structure and Development of the Tissues and Organs of the Body	50
A. De Watteville, for Investigations of Phenomena produced in living Tissues by a Galvanic Current, related apparently to the "Internal Polarisation of Dubois-Reymond"	30
Dr. F. Warner, to perfect a Method of obtaining Records of Muscular Movements in Adults and in Infants, especially those of the Hand and Arm	80
C. A McMunn, for Researches into the Colouring Matters of Bile and Urine, and for the purchase of a Spectroscope, an Electric Lamp, Animals, and other Materials	50
L. C. Wooldridge, for a Research into the Physiology of the Blood, especially of White Blood-corpuscles	30
Carried forward	£2,195

Brought forward	£2,195
A. Waller, for continuation of Investigations of the Electromotive Properties of Animal Tissues	50
Prof. Humpidge, for further Researches on Metallic Glucinum, to prepare larger quantities of the Metal, to determine its specific heat, and the Vapour-density of its Chloride and of its Compounds with Ethyl and Propyl	50
Dr. G. H. Morris, for Investigation of the solid Products of the destructive Distillation of Resin	25
Prof. W. N. Hartley, for continuation of Researches on Ultra-violet Spectra	100
R. H. M. Bosanquet, for the carrying out of work in the Laboratory of St. John's College, Oxford, viz., further Development and Applications of the Uniform Rotation Machine, Experiments in Magnetism and on the Intensity of Sound	100
G. Gore, for Experimental Investigations of the relations of Heat to Voltaic Actions and Thermo-electric Actions of Metals in Electrolytes	150
G. E. Dobson, for continuation of his illustrated Monograph on the Anatomical Structure, Systematic Position, and Geographical Distribution of the Species of the Order Insectivora	100
H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association	150
J. Murray. This sum to be placed at the disposal of one or two Members of the Committee for the procuring of certain Vertebrates in their early Stages, especially Ceratodus, Echidna, Ornithorhynchus, Halicore, and Callohrynchus	200
Rev. A. E. Eaton, to defray the cost of completing the Illustrations of a Monograph on the Ephemeridæ	100
Adam Sedgwick, for the expense of procuring a supply of well-preserved Embryos of *Peripatus capensis*	100
W. H. Caldwell, for assistance in studying the Development of *Ceratodus monotremata* and Marsupials in Australia	200
W. Topley, for a second instalment of the Grant towards the cost of the preparation and publication of a Geological Map of Europe, under the authority of the International Geological Congress	100
W. M. F. Petrie, for the cost of publication of an accurate Survey of the three Pyramids of Ghizeh and surrounding remains, with a Discussion of the Instrumental, Mathematical, and Historical questions connected with the Buildings	100
Prof. W. K. Parker, for assistance in his Researches into the morphology of the Vertebrata, more especially of the Skull	300
Carried forward	£4,020

Brought forward............	£4,020
W. Saville Kent, for continuation of researches in connexion with the sub-kingdom Protozoa	100
Prof. Lapworth, for aid in studying the Lower Palæozoic Rocks and Fossils of Britain	50
	£4,170

Dr. | *Cr.*

	£	s.	d.		£	s.	d.
To Balance on hand, Nov. 30, 1882	260	13	7	By Appropriations, as above............	4,170	0	0
To Grant from Treasury	4,000	0	0	Printing, Postage, Advertising, and other Administrative Expenses	63	6	4
To Repayments	173	0	0	Balance on hand, Nov. 30, 1883	232	18	9
To Interest on Deposit.........	82	11	6				
	£4,466	5	1		£4,466	5	1
Dec. 1, 1883. To Balance, and Moiety receivable from the Treasury.......	£2,232	18	9				

Account of Grants from the Donation Fund in 1882–83.

	£
The Paymaster-General, for Borings to ascertain the depth of the Fluviatile and other Deposits of the Middle and Lower Parts of the Delta of the Nile................................	150
Mr. Warren De La Rue, for the completing of his Catalogue of Latitudes and Longitudes of Solar Spots—£200.. on account	25
P. Herbert Carpenter, for preparation of a Monograph on the Blastoidea..	20
Dr. H. Gadow, for the cost of Appliances for the Breeding of Reptiles in Captivity	20
Mr. Forbes, for the purchase of the Ethnological Collection made by him during his expedition to Timor Laut and the neighbouring Islands....................................	40
The Linnean Society, to aid in the publication of Mr. Gosse's Paper "On the Prehensores of Male Butterflies of the Genera *Ornithoptera* and *Papilio*"..............................	50
	£305

Report of the Kew Committee for the Year ending October 31, 1883.

The operations of the Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. De La Rue, *Chairman.*

Captain W. de W. Abney, R.E.	Vice-Adm. Sir G. H. Richards, C.B.
Prof. W. G. Adams.	The Earl of Rosse.
Capt. Sir F. Evans, K.C.B.	Mr. R. H. Scott.
Prof. G. C. Foster.	Lieut.-General W. J. Smythe.
Mr. F. Galton.	Lieut.-Gen. R. Strachey, C.S.I.

Mr. E. Walker.

The Committee regret to announce the decease of their venerable Chairman, the late Sir E. Sabine, K.C.B., who died on the 26th of June at the very advanced age of ninety-four years and eight months. Sir Edward was one of the chief promoters of the Observatory, and took a leading part in its direction from its first establishment as a Physical Observatory in 1841. Up to within a few weeks of his death he was constant in his inquiries after its condition, although for the last eight years he had been prevented by infirmity from taking an active part in the meetings of its Committee.

The instruments employed for the absolute observations of terrestrial magnetism, as well as several less important pieces of apparatus used in the regular work of the Observatory, were originally provided by Sir E. Sabine.

To speak of that branch of their late Chairman's scientific work, with which the Kew Committee has been more particularly concerned, they may say that, in their opinion, the science of terrestrial magnetism owes more to Sir Edward Sabine than to any man who ever studied it, Gauss alone perhaps excepted.

The work at the Observatory may be considered under six heads:—

1st. Magnetic observations.

2nd. Meteorological observations.

3rd. Solar observations.

4th. Experimental, in connexion with any of the above departments.

5th. Verification of instruments.

6th. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have been in constant operation throughout the year.

The values of the ordinates of the different photographic curves determined in January were as follows :—

Declination : 1 inch=0° 22′·04. 1 mm.=0° 0′·87.

Bifilar, January 9, 1883,	for 1 inch	δH=0·0221 foot grain unit.
	,, 1 mm.	,, =0·0004 mm. mgr. unit.
Balance, January 12, 1883	,, 1 inch	δV=0·0376 foot grain unit.
	,, 1 mm.	,, =0·0007 mm. mgr. unit.

It having been decided to attempt to re-adjust the Bifilar and Vertical Force instruments so as to bring their scales more closely in accordance with the generally adopted values, the necessary alterations were made in the adjustments, and on redetermining the scale values on January 17th the following results were obtained:—

Bifilar	for 1 inch	δH=0·0277 foot grain unit.
	,, 1 mm.	,, =0·0005 mm. mgr. unit.
Balance	,, 1 inch	δV=0·0261 foot grain unit.
	,, 1 mm.	,, =0·0005 mm. mgr. unit.

The tabulation of the traces of the three elements was temporarily suspended at the conclusion of the year for which the observations were promised to the International Polar Commission. Attention is now being devoted to the revision and reduction of the results.

A common gas-jet has been substituted in the Vertical Force instrument with advantage for that formerly employed, but on trial the results obtained by a similar substitution in the case of the other instruments were not satisfactory, and the old burners with chimneys are retained for the present.

Gelatino-bromide paper has been used for all three instruments with great success and economy of time throughout the whole year, and an examination of the curves shows that not a single hour's trace has been lost since its adoption, from purely photographic causes, rapid and minute movements of the needles being recorded.

Several magnetic storms have been observed, the principal being that of November 17th and 18th, 1882, which, together with its accompanying aurora and meteor, excited considerable attention.

Owing to long usage the points of the Dip-needles Nos. 1 and 2 of Circle No. 33, used for monthly observations, were very blunt; they were accordingly re-ground by Mr. Dover in August, and the axles at the same time repolished.

The Committee have to acknowledge with thanks the receipt of photographic copies of magnetic curves from the Observatory at Batavia.

The magnetic instruments have been studied, and a knowledge of their manipulation obtained by—

Dr. Doberck.
Lieutenant A. P. Pinheiro.
Dr. O'Reilly.

Information on matters relating to terrestrial magnetism, and various data, have been supplied to Dr. Buys Ballot, Padre Denza, the Rev. F. Howlett, M. l'Abbé Philippe, and others.

The Unifilar Magnetometer returned by Rev. S. J. Perry, F.R.S., on his arrival in this country from Madagascar was lent to Professor W. G. Adams, F.R.S., for use in the Wheatstone Laboratory, King's College, London. Another Unifilar and Dip-circle have been lent to Professor O. Lodge, for use in the University College, Liverpool.

A Dip-circle with bar-magnets has been lent to Dr. E. van Rijckevorsel for use in an expedition to Central America.

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of	Horizontal Intensity	35
„	Dip	123
„	Absolute Declination	29

At the request of the Rev. S. P. Ferrari, of the private astronomical observatory on the Janiculan Hill, Rome, the superintendent designed a set of Magnetometers, for eye observations, on a new pattern, much less costly than the Kew magnetographs.

They were erected in the Verification House, and after a satisfactory trial, were dismounted and forwarded to M. l'Abbé Philippe for the Observatory at Lyons.

A set of Magnetographs has been ordered on behalf of the American Government, and is now in process of construction.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, and humidity,

wind (direction and velocity), sunshine, and rain, have been maintained in regular operation throughout the year. A summary of these observations is given in Appendix II.

Owing to the necessity of delaying the construction of the new tabulating scales for the wet bulb thermograph until a somewhat lengthened series of observations had been obtained, the work of tabulation became some months in arrear. These, however, have now been worked up, and the tabulations are up to date.

The standard eye observations for the control of the automatic records have been duly registered during the year, together with the daily observations at 0 h. 8 m. P.M. in connexion with the Washington synchronous system.

Owing to the high quality of the photographic records, the Committee considered that the maintenance of the noon eye observation in addition to that at 0 h. 8 m. was superfluous, and accordingly it was discontinued on July 1st.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted weekly to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	1825
,, dry and wet thermometers	3650
,, maximum and minimum thermometers	730
,, radiation thermometers	3599
,, rain gauges	730
Cloud and weather observations	2176
Measurements of barograph curves	9046
,, dry bulb thermograph curves	9786
,, wet bulb thermograph curves	11180
,, wind (direction and velocity)	17410
,, rainfall curves	864
,, sunshine traces	2252

In compliance with a request made by the Meteorological Council to the Kew Committee, the Observatories at Aberdeen, Stonyhurst, and Valencia, have been visited and their instruments inspected by Mr. Baker during his vacation.

With the concurrence of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by "The Times," "The Illustrated London News," "The Torquay Directory," and "The Torquay Standard," and data have been supplied to the Council of the Royal Meteorological Society, the

editor of "Symons's Monthly Meteorological Magazine," the Secretary of the Institute of Mining Engineers, Messrs. Banner and Co., the late Mr. Greaves, and Messrs. Gwilliam, Mawley, Rowland, Dr. Radcliffe, and others. The cost of these abstracts is borne by recipients.

Tracings of rain-gauge curves have been supplied to Mr. Symons for the months of October, November, and December, 1882.

Electrograph.—This instrument has been in continuous action through the year.

In May it was dismounted from the 16th to the 20th, to allow of structural alterations in connexion with the new stairs.

The tabulation of the curves is at present in arrear, not having been completed beyond February 28, 1882.

The portable Thomson Electrometer has not been employed in systematic observations during the year, but has had its scale value experimentally determined, at Mr. De La Rue's laboratory, for tensions ranging from -1240 to $+1030$ volts.

Information as to the working of Atmospheric Electrometers has been given to Professor Atwater, of Middleton, U.S.A., Dr. H. B. Baker, Lansing, U.S.A., and M. Leon Descroix, of Paris.

III. Solar Observations.

The sketches of Sun-spots as seen projected on the photoheliograph screen, have been made on 214 days, in order to continue Schwabe's enumeration, the results being given in Appendix No. II. The sun's surface was found to be free from spots on seven of those days.

Solar Negatives.—The correction to the area-measurements for foreshortening, which, at the date of the last report, had not been applied to the reductions of sun-spot observations for the last two years of the series, has since been made under Mr. De La Rue's direction.

The whole series is at the Royal Society, and is now being revised and arranged for reference by Mr. Marth, on behalf of the Council of the Royal Society, who made a grant of money for that purpose.

With the view of utilising the instrument in the transit of Venus of December 5, 1882, the Committee obtained the services of Mr. Reynolds, so long associated with Astronomical Photography, who made every preparation for taking a series of pictures of the transit. The adverse atmospheric conditions which prevailed at the time of the phenomenon, however, prevented any results being obtained.

Dr. Terby, of Louvain, requested a number of photographs, which were selected from the Kew series and sent him on loan. He has since returned them to the Observatory, having embodied the results

of his investigations in a work entitled "Sur l'Existence et sur la Cause d'une Périodicité mensuelle des Aurores Boréales."

Three typical negatives have also been selected, in reply to Professor Pickering's request, and forwarded to the Harvard College Observatory, to be deposited in the collection of astronomical photographs being formed there by the Director.

At the request of Professor Balfour Stewart, some measurements were made by Mr. Whipple and Mr. McLaughlin, in the Library of the Royal Astronomical Society, Burlington House, of Carrington's original sun-spot drawings, with a view of checking the accuracy of the values of solar-spotted areas determined at the Observatory in 1866, and published by Messrs. De La Rue, Stewart, and Loewy in their "Researches on Solar Physics," second series. The work has not yet been completed.

Transit Observations.—One hundred and fourteen observations have been made of sun-transits, for the purpose of obtaining correct local time at the Observatory; 224 clock and chronometer comparisons have also been made.

Shelton's clock, K.O., has been used as the standard timepiece of the Observatory.

IV. Experimental Work.

Actinometry.—Observations have been made on favourable occasions with the Stewart actinometer on the Observatory lawn, and the results communicated to the Meteorological Council, who defray the cost they entail. Owing to the rarity at Kew of the occurrence of periods of perfectly clear sky sufficient in duration for a satisfactory experiment with Stewart's apparatus, the Superintendent has instituted inquiries with a view to obtaining one of Professor Langley's bolometers for comparison with it.

Solar Radiation Thermometers.—With a view of investigating the causes of the differences in the readings of black bulb thermometers in *vacuo*, the Superintendent obtained on loan from Messrs. Negretti and Zambra six of these instruments constructed according to his suggestions. They were after verification arranged on a stand on the Observatory lawn beside the Observatory standard of reference, and read daily during the summer months. The observations have been discussed, and the results indicate that the discrepancies observed in the readings of this class of instrument are in part to be attributed to want of uniformity in the sizes of the thermometer bulbs, and also in the amount of lampblack with which they are covered.

Photo-Nephograph.—At the request of the Meteorological Council, a series of experiments have been commenced with Captain Abney's Photo-Nephograph, described in the Report of the Council for 1881.

Two of the cameras, with their tripod stands, have been received at the Observatory, a base line of 180 yards has been marked off on the level path leading across the park from the Observatory, and a carriage for conveying the battery and reels of wire constructed.

A code of signals has been arranged to enable the observers at the cameras to work in accordance with each other, and several successful pairs of cloud negatives have been obtained, both on the plates prepared by Captain Abney and also on gelatino-bromide paper.

No steps have yet been taken towards the permanent installation of the apparatus at the Observatory.

Water Surface Temperature.—The observations of the maximum and minimum temperature of the surface water of the pond which were taken for the late Mr. Greaves, C.E., daily at 9 A.M., were discontinued at his request on May 1, and the results forwarded to him.

Mr. Greaves applied to the Committee for permission to excavate a tank in the ground attached to the Observatory, in order that continuous registration of water surface temperature might proceed in the immediate neighbourhood of the thermograph. The Committee, however, were unable to afford him the facilities he desired, the time of the Observatory staff being fully occupied with their existing duties.

Nocturnal Radiation.—The experiments on the fall of temperature of the lower layers of the atmosphere at sunset, instituted at the suggestion of Professor Tyndall, were terminated on February 16, on the resignation of the assistant by whom the readings of the thermometers were made, the grant devoted by the Meteorological Council to the purpose being almost expended.

Graphic Reductions.—The Superintendent, having made some experiments on the deduction of mean values, &c., from curves by a graphic method, based on Mr. Galton's composite portraiture, has communicated a paper on the subject to the Royal Meteorological Society, which has been published in the "Quarterly Journal," vol. ix.

Artificial Horizon.—Some experiments have been made with a view of testing an attachment to sextants answering the purpose of an artificial horizon, which has been invented by Mr. T. Tennent, of San Francisco, and constructed by Messrs. Elliott Brothers. The results appear to indicate that the invention will prove a useful addition to a sextant under certain conditions.

Watch-rating.—The Committee, having decided to make a trial of a system of watch-rating for the public, have granted £100 for the preliminary expenses. In accordance with a scheme prepared by the Superintendent, they have fitted up at the Observatory a first-class burglar- and fire-proof safe for the safe custody of the watches, and with a view to the obtaining of star-transits, have permitted Mr. Whipple to fit up a temporary transit-house at his residence in the

neighbouring town of Richmond, where he has erected a Sheepshanks 30-inch transit (No. 27) lent by the Royal Astronomical Society. The apparatus used for determining the temperature correction of aneroids is being fitted up to receive the watches for rating them at extreme temperatures, and arrangements are in progress for their reception and delivery at the Meteorological Office by Mr. Strachan, and at the Horological Institute, Northampton Square, by Mr. Britten, the Secretary.

A circular has been drafted, which will be issued to watchmakers on the completion of these arrangements, and it is hoped that operations will be commenced early in the new year.

Pendulum Experiments.—Professor C. S. Peirce, of the United States Coast Survey, who made a series of pendulum observations at Kew and elsewhere in 1876, visited the Observatory in July last, and made a subsidiary series of experiments with a view of determining the flexure of his stand when on the Kew piers, using for the purpose an instrument termed a "noddy."

Major J. E. Herschel, R.E., F.R.S., and Mr. Chaney, of the Standards Department, visited the Observatory and witnessed some of his experiments.

V. Verification of Instruments.

The following magnetic instruments have been verified, and their constants have been determined:—

6 Unifilar Magnetometers for Elliott Brothers, London.
2 Dip Circles for Casella, London.
1 Dip Circle for Dover, Charlton.

There have also been purchased on commission and verified:—

A Unifilar Magnetometer for Professor Brioschi, Naples.
A Unifilar Magnetometer for Professor Ferrari, Rome.
A Dip Circle for Professor Thalén, Upsala.
A Dip Circle for Professor Ferrari, Rome.
A complete set of Magnetometers for the Lyons Observatory, France.

Two Dip Circles are at present undergoing examination:—

The number of meteorological instruments verified continues still to increase, having been in the past year as follows:—

Barometers, Standard	45
„ Marine and Station	114
Aneroids	52
Total	211

Thermometers,	ordinary Meteorological	1165
"	Standard	116
"	Mountain	39
"	Clinical	7255
"	Solar radiation	35
	Total	8610

Besides these, 51 Deep-sea Thermometers have been tested, 2 of which were subjected in the hydraulic press, without injury, to pressures exceeding three and a half tons on the square inch, and 78 Thermometers have been compared at the freezing-point of mercury, making a total of 8739 for the year.

Duplicate copies of corrections have been supplied in 17 cases.

The number of instruments which were rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers,	clinical	19
"	ordinary meteorological	4
Barometers		31

Seven Standard Thermometers have also been calibrated and divided, and supplied to societies and individuals during the year.

A Barograph and Thermograph have been examined, and had their scale values determined for the Hong Kong Observatory, also a Barograph for the Japanese Hydrographic Department, and a large Anemograph for the Zi-Ka-Wei Observatory.

The following miscellaneous instruments have also been verified:—

Hydrometers	59
Anemometers	12
Rain Gauges	9
Theodolite	1
Sextants	55
Index and Horizon Glasses, unmounted	111
Dark Glasses, unmounted	277
Prismatic Compasses	2
Marine Telescopes	3

There are at present in the Observatory undergoing verification, 27 Barometers, 932 Thermometers, 1 Anemometer and 4 Sextants, and a self-registering Aneroid for the Meteorological Council.

The Committee have recently revised the regulations for the verification of graduated instruments, fixing a linear value equal to 0·01 inch or 0·25 millim. as the limit to which corrections are to be assigned to scales intended to be read by the unassisted eye.

With a view of facilitating the examination of the dark glasses and mirrors of sextants, of which a large number are now tested and marked for makers before mounting in frames, the Superintendent has devised a special apparatus for the purpose, an illustrated description of which appeared in the "Proceedings of the Royal Society" (vol. 35, p. 42).

Redeterminations have been made of the angles between the collimators of the Cooke sextant apparatus, which show that they retain their positions with a satisfactory degree of constancy.

The Committee have been offered the loan of the apparatus employed by Mr. J. M. Crafts, of Paris, for the comparison of mercurial thermometers at high temperatures, but have not yet been able to avail themselves of his offer.

Standard Barometers.—From time to time comparisons have been made between the two Welsh Standard Barometers and Newman No. 34, the working Standard of the Observatory, and their relative values have been found to remain unchanged.

Mr. F. Waldo, of the United States Signal Department, being instructed by Major-General W. B. Hazen, Chief Signal Officer, United States of America, to compare the Standard Barometers of their Department with the European Standards, visited the Observatory in July, and made a lengthened comparison of two Standards by Fuess, which he brought with him, with the Observatory Working Standard, Newman No. 34. The results of his comparison have not yet been communicated to the Committee, but Dr. Chistoni, of the Italian Meteorological Service, having published in the "Annale della Meteorologia" an account of the results of his comparisons of Kew and other Standard Barometers, the Committee desire to publish an abstract of that part of his paper which more especially refers to the Observatory Standard.

Taking the absolute standard barometer of St. Petersburg as the basis for his comparisons, Dr. Chistoni finds that the corrections of the Continental standards, referred to this instrument, are as follows:—

Barometer.	Millim.	In.
Hamburg, Fuess, No. 9	−0·35	−0·014
" " No. 5	−0·14	−0·006
" " No. 10	−0·22	−0·009
Berlin, Old Standard, by Greiner	−1·16	−0·046
Copenhagen, Jünger	−0·11	−0·004
Rome, Deleuil, No. 6	−0·22	−0·009
Stockholm, Pistor and Martins, No. 579	0·00	0·000
Vienna, Pistor	−0·17	−0·007

His first comparison with the Kew Standard was an indirect one,

made by means of a Negretti and Zambra's Standard, No. 1042, which he found at Pesaro, and which had been compared at Kew in 1877. By means of an indirect comparison of this instrument with the Standard, Deleuil No. 6, at Rome, he found the correction of the latter, so referred to the Kew Standards, to be −0·23 millim. (−0·009 inch). Subsequently, Professor Tacchini conveyed another barometer directly to Kew, and this when compared with the Roman Standard, indicated the difference between the two instruments to be −0·19 millim. (−0·008 inch). From these two comparisons he assumes the true correction of Deleuil No. 6 to be −0·21 millim. (−0·008 inch). Having already determined the correction of that instrument referred to the St. Petersburg Standard to be −0·22 millim. (−0·009 inch), he concludes that the two absolute Standard Barometers of Kew and St. Petersburg perfectly agree, and taking into account the possible error of reading the instrument at Rome, "they cannot differ between themselves more than half the tenth of a millim. (0·002 inch)." With the absolute Standard of the Collège de France he found the correction of Deleuil No. 6 to be −0·18 millim. (−0·007 inch). Hence he also concludes that the absolute Standard Barometer of the Collège de France perfectly agrees with the absolute standards of Kew and St. Petersburg within the limits of the half-tenth of a millim. (0·002 inch).

VI. Miscellaneous.

Waxed Papers, &c., supplied.—Waxed paper has been supplied to the following Observatories :—

Colaba, Toronto, Mauritius, and the Meteorological Office.

Anemograph Sheets have been sent to Mr. Pogson, Madras Observatory.

Blank Magnetic Observation Forms have been supplied to Professor Brioschi, Naples.

Two glass tabulating scales for measuring magnetograph curves were constructed for the Toronto Observatory, and five various glass scales for the Hong Kong Observatory.

Two Additional Divided Plates for the Sun Picture Micrometer have been supplied to the Mauritius Observatory.

A Thomson Quadrant Electrometer was procured from the maker, and after examination, forwarded to Senhor Capello, of Lisbon. A portable Thomson Electrometer has also been purchased, and had its scale value determined for M. le Directeur de l'Institut Technique et Nautique de Bari, Italy.

Magnetic Survey of Great Britain and Ireland.—The attention of the Committee having been called to the fact that twenty-four years have elapsed since the surveys of Sabine and Welsh were completed, and

that a new survey is now desirable, they have requested the Hydrographer (Sir F. Evans), Professors W. G. Adams and G. Carey Foster to act as a sub-committee with a view of recommending the course to be adopted for the carrying out of the survey.

A number of instruments of interest were exhibited at the Fourth Annual Exhibition of the Royal Meteorological Society, which was devoted to meteorological instruments used by explorers and travellers, held in the rooms of the Institution of Civil Engineers in March last.

Library.—During the year the Library has received, as presents, the publications of—

27 English Scientific Societies and Institutions, and

88 Foreign and Colonial Scientific Societies and Institutions.

153 Volumes of duplicates of works on Astronomy, Terrestrial Magnetism, and Meteorology, have been presented to the Library of the newly established Observatory at Hong Kong.

Observatory and Grounds.—The buildings and grounds have been kept in order throughout the year. A new staircase leading from the ground to the first floor has been constructed; a new chimney fitted to the barometer-room stove, and the exterior of the building maintained in repair by Her Majesty's Commissioners of Works, &c.

A temporary vestibule has been put up in the Entrance Hall.

Owing to the giving way of a gas-pipe support attached to the Electrograph, the building narrowly escaped being set on fire on the night of March 9th; the housekeeper fortunately being near at the time, the fire was extinguished without damage being done. Steps have been taken to prevent a recurrence of the accident.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant and Magnetic Observer.
J. Foster, Verification Department.
H. McLaughlin, Librarian and Accountant.
E. G. Constable, Solar Observations and Tabulation of Meteorological Curves.
T. Gunter, Verification Department.
W. Boxall, Photography.
E. Dagwell, Office duties.
E. Coates, C. Henley } Verification Department.
M. Baker, Messenger and Care-taker.

During the Spring, Mr. Whipple met with an accident which

entailed his absence from the Observatory for three months on a medical certificate, during the interval Mr. R. H. Scott undertook the general supervision of its affairs, Mr. T. W. Baker, as chief assistant, conducting the work at the Observatory.

Mr. F. G. Figg having been appointed first assistant in the Hong Kong Observatory, left at the end of June, and Mr. T. W. Baker undertook the duties of Magnetic Observer, Mr. Foster taking charge of the Meteorological Instruments Verification Department.

Mr. Dawson, messenger and caretaker, resigned in March on account of ill-health. Messrs. C. Taylor and S. Henley have also resigned. H. Clements was temporarily re-engaged in the Verification Department for six weeks.

Abstract. *Kew Observatory Receipts and Payments Account from November 4, 1882, to November 1, 1883.*

Dr. **RECEIPTS.**

	£ s. d.	£ s. d.
To Balance from 1881–82		504 15 0
Royal Society (Gassiot Trust)		493 11 1
Meteorological Office		400 0 0
Meteorological Office, for Researches, Postages, &c.		9 5 1
Verification Fees, Meteorological Office	£91 3 0	
" " Observatories and Institutions	23 5 6	
" " Instrument Makers and others	501 10 0	
		615 18 6
Instruments on Commission		360 6 8
Sale of Waxed Paper		68 10 11
Standard Thermometers	15 17 6	
" Forms, &c.	2 14 5	
Copying Registers	79 13 6	
		98 5 5
Mr. De La Rue for Sun Work, &c.		15 8 3
Experimental Work		24 4 6
		£2590 5 5

PAYMENTS. *Cr.*

	£ s. d.	£ s. d.
By Salaries	£1035 17 10	
Extra Payments	200 11 10	
		1236 9 8
Fuel and Gas	61 6 3	
Furniture and Fittings	7 6 5	
Chandlery, &c.	7 19 2	
Painting and Repairs	21 2 4	
Rent and Incidentals (Enclosure and Path)	21 3 11	
		118 18 1
Printing and Stationery (General)	26 1 10	
" " (Verification Department)	26 4 8	
Postages	15 17 8	
Library	8 4 8	
Messengers and Housekeepers	71 11 0	
Porterage and Contingencies	20 19 9	
		168 19 7
Purchase of Chemicals and Materials	30 14 6	
" " Tubes for Standard Thermometers	3 15 0	
" " Anemograph Sheets	2 1 0	
Verification Department Expenses (Ice, Carbonic-Acid Gas, &c.)	11 8 2	
Repair of Instruments, and Purchase of New	13 14 11	
Carpenter's Work and Sundries	4 10 9	
		66 4 4
Postages and Payments on behalf of Meteorological Office		5 10 10
Instruments purchased on Commission		291 2 10
Purchase of Waxed Paper, Packing ditto, &c.		50 10 10
Payments on behalf of Sun Work		14 8 0
" " Experimental Work		10 8 8
" " Watch Rating		80 7 8
Balance—Bank of England	479 16 4	
London and County Bank	50 0 0	
Cash in hand	17 8 7	
		547 4 11
		£2590 5 5

November 24, 1883. **Examined and compared with the Vouchers, and found correct.** (Signed) R. STRACHEY, *Auditor.*

ASSETS.

	£ s. d.
By Balance as per Statement	547 4 11
Meteorological Office, Allowances and Sundries	50 5 1
Verification Fees due	82 1 0
Waxed Paper	30 0 0
Commissions	78 12 6
Blank Forms	9 8 0
Standard Thermometers	94 10 0
	£892 1 6

LIABILITIES.

	£ s. d.
To Gas, Fuel, and House Account	12 5 5
Apparatus, Chemicals, &c.	9 6 2
Commissions	134 6 4
Balance	736 3 7
	£892 1 6

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1882 *to September* 1883.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets N D and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are 3½ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00109$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have been made and reduced by Messrs. F. G. Figg and T. W. Baker.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$ Mean.
1882.	d. h. m.	foot.	°	° ′ ″	
October........	26 12 39 P.M.	1·0	49·8	15 27 30	
		1·3	....	6 58 28	9·12609
	2 35 „	1·0	51·7	15 27 12	
		1·3	....	6 58 10	
November......	28 12 41 P.M.	1·0	44·9	15 27 43	
		1·3	....	6 58 26	9·12583
	2 33 „	1·0	45·4	15 27 30	
		1·3	....	6 58 24	
December......	20 12 32 P.M.	1·0	37·9	15 28 1	
		1·3	....	6 58 40	9·12599
	2 23 „	1·0	38·8	15 29 46	
1883.		1·3	....	6 59 22	
January........	26 12 46 P.M.	1·0	45·1	15 27 10	
		1·3	....	6 58 11	9·12570
	2 32 „	1·0	46·5	15 27 21	
		1·3	....	6 58 18	
February	27 12 36 P.M.	1·0	46·2	15 26 45	
		1·3	....	6 58 1	9·12527
	2 27 „	1·0	49·1	15 25 6	
		1·3	....	6 57 29	
March	29 1 1 P.M.	1·0	53·8	15 25 41	
		1·3	....	6 57 50	9·12554
	2 40 „	1·0	55·1	15 24 59	
		1·3	....	6 57 28	
April..........	25 12 39 P.M.	1·0	58·5	15 26 24	
		1·3	....	6 57 40	9·12549
	2 37 „	1·0	60·6	15 23 10	
		1·3	....	6 56 35	
May	24 12 53 P.M.	1·0	77·1	15 20 26	
		1·3	....	6 55 12	9·12457
	2 44 „	1·0	80·5	15 19 12	
		1·3	....	6 54 41	
July	12 3 50 P.M.	1·0	67·4	15 19 30	
		1·3	....	6 55 13	9·12423
	13 12 2 „	1·0	70·9	15 21 33	
		1·3	....	6 55 22	
August	1 11 46 A.M.	1·0	67·9	15 22 2	
		1·3	....	6 55 51	9·12459
	3 32 P.M.	1·0	73·9	15 19 49	
		1·3	....	6 55 17	
August	22 12 31 P.M.	1·0	78·0	15 19 35	
		1·3	....	6 54 55	9·12431
	2 23 „	1·0	78·7	15 19 6	
		1·3	....	6 54 30	
October........	1 12 42 P.M.	1·0	57·0	15 22 24	
		1·3	....	6 56 13	9·12414
	2 18 „	1·0	58·1	15 20 52	
		1·3	....	6 56 9	

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.*	Log mX. Mean.	Value of m.†
1882.	d. h. m.	°	secs.		
October..........	26 12 1 P.M.	46·9	4·6500		
	3 8 P.M.	53·1	4·6525	0·30844	0·52151
November........	28 12 7 P.M.	43·5	4·6517		
	3 4 P.M.	45·4	4·6498	0·30824	0·52123
December........	20 11 50 A.M.	35·7	4·6478		
	2 58 P.M.	38·5	4·6505	0·30808	0·52109
1883.					
January..........	26 12 13 P.M.	43·9	4·6485		
	3 6 P.M.	46·8	4·6507	0·30848	0·52130
February	27 11 52 A.M.	45·0	4·6483		
	3 37 P.M.	52·0	4·6502	0·30875	0·52121
March...........	29 12 22 P.M.	52·6	4·6523		
	3 15 P.M.	55·0	4·6497	0·30873	0·52135
April............	25 11 56 A.M.	56·9	4·6566		
	3 15 P.M.	60·1	4·6521	0·30839	0·52112
May.............	24 12 6 P.M.	76·1	4·6534		
	4 6 P.M.	84·0	4·6543	0·30989	0·52147
July.............	12 2 52 P.M.	67·0	4·6518		
	13 12 38 P.M.	72·6	4·6540	0·30939	0·52097
August	1 12 26 P.M.	69·1	4·6532		
	2 57 P.M.	73·1	4·6534	0·30935	0·52116
August	22 11 51 A.M.	76·9	4·6575		
	3 14 P.M.	78·3	4·6543	0·30930	0·52095
October..........	1 11 51 A.M.	55·9	4·6542		
	3 3 P.M.	57·7	4·6500	0·30870	0·52050

* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

† m = magnetic moment of vibrating magnet.

Dip Observations.

Month.	G. M. T.	Needle.	Dip.	Month.	G. M. T.	Needle.	Dip.
			North.				North.
1882.	d. h. m.	No.	° ′	1883.	d. h. m.	No.	° ′
Oct.	30 3 12 P.M.	1	67 40·56	April	26 3 15 P.M.	1	67 39·37
	3 12 „	2	41·06		3 20 „	2	39·72
	31 3 12 „	1	40·75		27 3 30 „	1	39·81
	3 12 „	2	41·19		3 30 „	2	38·40
	Mean..	....	67 40·89		Mean..	....	67 39·32
Nov.	29 3 2 P.M.	1	67 41·56	May	29 2 48 P.M.	1	67 41·55
	3 1 „	2	40·56		3 45 „	2	41·49
	30 3 3 „	1	41·06		30 3 10 „	1	42·60
	3 5 „	2	40·56		3 32 „	2	39·74
	Mean..	....	67 40·93		Mean..	....	67 41·34
Dec.	22 2 58 P.M.	1	67 41·28	June	27 2 15 P.M.	1	67 41·62
	2 58 „	2	41·40		2 52 „	2	39·22
	Mean..	....	67 41·34		28 2 49 „	1	40·12
					3 26 „	2	37·91
					Mean..	....	67 39·72
1883. Jan.	29 3 44 P.M.	1	67 41·81	July	26 2 15 P.M.	1	67 40·25
	3 42 „	2	40·25		2 50 „	2	40·18
	30 3 18 „	1	41·50		28 4 40 „	1	38·28
	3 16 „	2	39·47		5 12 „	2	40·90
	Mean..	....	67 40·76		Mean..	....	67 39·90
Feb.	23 4 1 P.M.	1	67 42·50	Aug.	20 3 0 P.M.	1	67 40·46
	4 2 „	2	41·18		3 3 „	2	39·78
	26 3 13 „	1	40·50		21 2 42 „	1	39·19
	3 12 „	2	40·75		2 41 „	2	39·65
	Mean..	...	67 41·23		Mean..	....	67 39·77
Mar.	30 3 40 P.M.	1	67 41·21	Sept.	29 2 56 P.M.	1	67 40·94
	3 40 „	2	40·06		2 56 „	2	42·00
	31 3 0 „	1	40·93	Oct.	2 2 44 „	1	42·62
	3 1 „	2	39·81		2 45 „	2	40·15
	Mean..	....	67 40·50		Mean..	....	67 41·43

Month.	Declination.	Magnetic Intensity.								
	Mean of Observations.	English Units.			Metric Units.			C. G. S. Measure.		
		X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1882. October	18° 43′ 37″	3·9009	9·5028	10·2721	1·7987	4·3816	4·7363	0·1799	0·4382	0·4736
November	18 41 3	3·9012	9·5036	10·2733	1·7988	4·3820	4·7369	0·1799	0·4382	0·4737
December	18 39 51	3·9025	9·5100	10·2795	1·7994	4·3849	4·7397	0·1799	0·4385	0·4740
1883. January	18 38 1	3·9029	9·5065	10·2766	1·7996	4·3833	4·7384	0·1800	0·4383	0·4738
February......	18 38 51	3·9061	9·5179	10·2880	1·8010	4·3885	4·7438	0·1801	0·4388	0·4744
March........	18 40 36	3·9047	9·5089	10·2795	1·8004	4·3844	4·7397	0·1800	0·4384	0·4740
April	18 40 9	3·9035	9·4964	10·2674	1·7998	4·3787	4·7341	0·1800	0·4379	0·4734
May..........	18 35 44	3·9144	9·5390	10·3107	1·8048	4·3983	4·7541	0·1805	0·4398	0·4754
June	18 40 46	3·9136	9·5245	10·2972	1·8045	4·3916	4·7479	0·1804	0·4392	0·4748
July..........	18 50 23	3·9118	9·5214	10·2937	1·8037	4·3902	4·7462	0·1804	0·4390	0·4746
August	18 45 5	3·9128	9·5230	10·2953	1·8041	4·3909	4·7470	0·1804	0·4391	0·4747
September	18 42 4	3·9109	9·5313	10·3024	1·8033	4·3947	4·7503	0·1803	0·4395	0·4750

APPENDIX II.

Meteorological Observations.—Table I.

The Kew Observatory, Richmond, Surrey.

Longitude $0^h\ 1^m\ 15^s{\cdot}1$ W. Latitude 51° 28′ 6″ N. Height above sea-level =34 feet.

Mean Monthly results from the continuous Records for the Twelve Months ending September 30th, 1883.

Months.	Thermometer.* Means.	Means of max. temp.	Means of min. temp.	Means of max. and min. comb.	Extreme maximum. Date.	Extreme maximum. Ther.	Extreme minimum. Date.	Extreme minimum. Ther.	Barometer.† Means.	Extreme maximum. Date.	Extreme maximum. Bar.	Extreme minimum. Date.	Extreme minimum. Bar.	Means of vapour-tension.
1882.	°	in.	in.		d. h.	°	d. h.	°	inches.	d. h.	inches.	d. h.	inches.	inch.
Oct.....	50·6	56·8	44·4	50·6	1 4 P.M.	68·6	26 6 A.M.	29·9	29·842	4 10 P.M.	30·525	24 {11 A.M. / noon}	28·852	·318
Nov.	43·8	48·5	38·8	43·7	5 {1 „ / 3 „}	59·8	18 {4 „ / 7 „}	24·6	29·709	28 6 „	30·195‡	16 5 A.M.	29·121	·232
Dec.	40·3	44·1	36·0	40·1	27 1 „	57·1	11 5 „	22·4	29·678§	20 10 A.M.	30·341	4 1 „	29·057	·225
1883. Jan.....	41·8	45·8	37·2	41·5	1 {2 „ / 6 „}	55·0	31 4 „	29·3	29·915	23 9 „	30·670	26 3 „	29·083	·227
Feb.	43·0	48·5	37·9	43·2	22 4 „	54·9	17 8 „	29·4	30·086	23 {9 „ / 10 „ / 11 „}	30·860	2 4 P.M.	28·938	·238
March..	36·3	42·9	30·1	36·5	5 3 „	53·7	24 6 „	22·6	29·939	3 10 P.M.	30·722	26 4 A.M.	29·221	·162
April...	46·7	56·0	38·5	47·3	5 2 „	64·9	1 6 „	29·8	30·015	7 {7 A.M. / 8 „}	30·673	27 4 P.M.	29·410	·234
May ...	52·6	61·1	44·2	52·7	24 4 „	76·0	4 5 „	31·8	29·967§	17 8 „	30·403	9 4 A.M.	29·495	·283
June...	58·7	67·8	49·6	58·7	29 4 „	83·4	1 4 „	41·1	29·973	13 9 „	30·408	26 4 „	29·746	·354
July ...	59·7	68·0	52·0	60·0	2 3 „	82·3	16 4 „	44·1	29·868	16 2 P.M.	30·183	12 6 P.M.	29·475	·375
Aug....	61·7	70·8	53·4	62·1	21 2 „	79·2	12 4 „	46·0	30·022	19 8 A.M.	30·298	9 3 A.M.	29·588	·394
Sept....	56·7	64·9	50·2	57·6	19 2 „	73·6	9 6 „	40·0	29·836	13 9 „	30·276	2 4 P.M.	28·777	·376
Means..	49·3	56·3	42·7	49·5	....	..	....	..	29·904	....	..	....	..	·285

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometers are 10 feet above the ground.

† Readings reduced to 32° F. and to sea-level.

‡ Approximate reading.

§ Mean for one day is approximate.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *. Total.	Rainfall *. Maximum.	Rainfall *. Date.	Weather. Number of days on which were registered: Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Wind †. Number of days on which it was: N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
1882.		in.	in.																	
October ..	7·6	5·770	0·845	15	23	..	1	..	..	16	1	3	5	1	4	4	7	3	4	7
November	6·9	2·340	0·375	15	22	1	1	..	..	14	2	3	2	1	..	2	9	8	5	1
December	8·4	2·115	0·420	7	19	3	..	..	..	17	..	5	..	1	4	4	8	6	3	5
1883.																				
January ..	8·0	2·215	0·535	15	22	..	..	..	..	23	4	1	3	6	3	2	11	4	1	..
February .	7·3	3·415	0·570	10	18	..	..	..	3	16	3	..	..	2	1	6	12	5	2	3
March ...	6·1	0·965	0·370	19	6	7	..	..	4	12	4	8	8	2	..	2	2	3	6	1
April	6·4	1·620	0·525	18	7	1	..	..	3	13	..	7	3	5	2	3	3	5	2	8
May	6·2	1·830	0·475	11	10	..	..	..	4	11	..	7	3	..	3	2	10	3	3	8
June	6·4	1·165	0·300	26	13	..	1	2	4	12	..	3	6	2	3	2	9	2	3	8
July	7·2	2·030	0·540	14	14	..	2	5	..	18	..	2	..	..	2	2	12	7	6	4
August ..	6·1	0·930	0·490	31	11	..	..	1	5	11	..	3	..	2	2	1	10	9	4	11
September	6·5	3·285	0·775	29	16	..	..	..	1	14	1	4	5	1	1	5	5	8	1	7
Totals..		27·680			181	12	5	8	24	177	15	46	35	23	25	35	98	63	40	63

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the Air.*			
	Total number of hours recorded.	Percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Hour.	Date.
1882.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.		
October	78·42	24	8 36	2	93	118	2	40·8	25·9	30	9	38	1 P.M.	24
November ..	75·12	28	6 12	11	80	104	3	34·0	19·0	18	15	39	9 & 10 P.M.	3
December ..	23·42	10	5 6	4	59	79	4	32·8	18·0	10	10	31	2 A.M.	29
1883.														
January	46·18	18	5 36	26	66	87	28	32·7	20·8	24	13	48	11 A.M.	29
February	71·48	26	8 54	23	83	106	28	32·5	20·9	17	12	38	7 & 8 P.M.	2, 10
March	141·54	39	10 42	25	93	115	31	24·9	11·9	24	14	38	1 & 2 P.M.	6, 22
April	155·36	38	10 48	17	109	128	30	32·1	19·1	9	9	29	9 & 11 A.M.	6, 18
May	205·54	43	14 48	24	118	133	31	38·7	25·2	4	8	30	8 A.M. & 1 P.M.	1, 13
June	186·12	38	14 48	3	125	141	29	44·7	30·6	17	8	27	10 A.M.	3
July	168·12	35	12 36	1	128	137	2	45·3	35·3	16	9	31	1 P.M.	11
August	180·18	40	12 36	10	124	135	21	47·4	36·4	20	8	32	2. 3, & 4 P.M.	14
September ..	129·00	34	9 54	9	112	127	1	45·8	33·3	9	9	34	9 A.M.	2

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1882.			
October	15	10	1
November	20	20	0
December	10	7	0
1883.			
January	13	14	0
February	14	10	1
March	22	13	2
April	21	15	0
May	23	10	2
June	22	12	0
July	19	14	0
August	16	11	0
September	19	13	1
Totals	214	149	7

APPENDIX III.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Old Kew Thermometer Screen Portable Transit Instrument	1868 1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Dr. T. Thorpe, F.R.S.	Three Open Scale Standard Thermometers, Nos. 561, 562, and 563. Tripod Stand	1879 1883
Major Herschel, R.E., F.R.S.	Invariable Pendulums, Nos. 1821, 4, and 11, Shelton Clock, R.S. No. 34. Stands, and Accessories.	1881
Mr. R. W. Munro	Standard Straight-edge	1881
Capt. Dawson, R.A.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. Two Bifilar Magnetometers. One Balance Magnetometer. Two Declinometers. Two Tripod Stands.	1882
Major-General Sir H. Lefroy, R.A., F.R.S.	Two parcels Magnetical and Meteorological MSS. from the Sabine Magnetic Office.	1882
Dr. E. van Rijckevorsel	Dip-Circle by Barrow, No. 24, complete, with four Needles, and a Pair of Magnetizing Bars.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
Mr. E. Mawley	Small Air Meter, with Robinson's Cups	1883
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883

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"On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting them when Magnetised, or by Magnetising them when Twisted." By J. A. EWING, B.Sc., F.R.S.E., Professor of Mechanical Engineering and Physics in the University of Tokio, Japan, now Professor of Engineering in the University College, Dundee. Received October 21, 1882.*

§ 1. A chance observation, made in the summer of 1881, drew my attention to the fact that when the ends of an iron wire are connected to the terminals of a galvanometer, a transient current will be observed if the wire be suddenly magnetised while it is held in a state of torsion, or suddenly twisted whilst in a state of longitudinal magnetisation. Further inquiry showed that these currents are satisfactorily explainable by reference to the results of more direct observations which have been made on the effects of stress on magnetism. They present, however, certain features of interest, and the examination of them, of which a condensed account is given below, may form a useful supplement to any direct investigation of the effects of stress on magnetic susceptibility and residual magnetism.

§ 2. A straight, moderately soft, well-annealed iron wire, 1 millim. in diameter and 34 centims. long, was placed horizontally in an E. W. position, with one end securely fixed and the other held by a twisting arm, by which any desired amount of torsion might be applied. The twisting arm was provided with a pointer, which travelled over a graduated circular dial. The ends of the wire were connected by long leading wires to a Thomson's mirror galvanometer of 0·25 ohm resistance, with a heavy mirror, which made it fairly suitable for ballistic work. Round the iron wire was a magnetising solenoid of 660 turns, 31 centims. long, in two layers, wound so as to have no longitudinal component parallel to the wire. In circuit with the solenoid was a single large Daniell's cell and a reversing key. The resistance of this

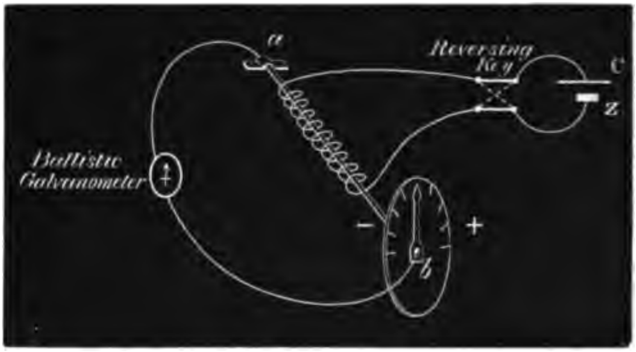
* This condensed version of a paper with the above title, read before the Royal Society, November 17, 1881 (see "Proceedings," vol. 33, p. 21), together with a Supplementary Note received later, is printed by order of the Committee of Papers.—G. G. S.

circuit was 1·36 ohms and, if we assume the electromotive force of the cell to have been 1 volt, the magnetising force was therefore

$$4\pi \times \frac{660}{31} \times \frac{1 \times 10^8}{1{\cdot}36 \times 10^9} = 20 \text{ c.g.s. units very nearly.}$$

The ideal diagram (fig. 1) will help in describing the directional relation of the transient currents, the magnetisation, and the torsion.

Fig. 1.



There *ab* is the iron wire, with the twisting-arm and dial-plate at *b*. The two directions of twist are distinguished by the signs + and −, the latter being used when the twist is that of a common screw. The two directions of the magnetising current will be called A and B; the connexions for A are shown by the full lines, and for B by the dotted lines. A has the effect of making the dial end (*b*) of the wire a nominal N. (*i.e.*, a "north-seeking") pole. A transient current will be called positive when it flows along the wire from *a* to *b*.

§ 3. When the wire was in a state of no torsion the closing of the battery circuit did not of course produce any current in the ballistic galvanometer; nor, when the wire was sensibly free from magnetisation, did twisting it produce any effect. But when the wire was first twisted negatively (like a common screw) and then magnetised by closing the circuit, A, a transient current flowed along it from *b* to *a*, that is, from N. to S. Reversal of the magnetising current from A to B, the wire being still held twisted, caused a transient current of twice the quantity of the first to pass in the opposite direction.

In like manner, if the magnetising current A was first made and the wire then twisted suddenly like a common screw, a transient current flowed along it from *b* to *a*, that is from N. to S., and similar currents were observed when, without maintaining the magnetising force in action, a permanently magnetised wire was suddenly twisted.

§ 4. The existence of these transient currents is due to the production of a state of circular magnetisation as the result of torsional stress and longitudinal magnetism acting jointly. To prove this, I substituted for the wire a long piece of iron gas-pipe, itself insulated, but carrying in its interior an independent copper wire, which was in circuit with the ballistic galvanometer. The gas-pipe was longitudinally magnetised by a surrounding solenoid. The making and reversing of a current in the solenoid whilst the pipe was held twisted, or the sudden twisting of the pipe during or after the operation of the magnetising force, gave transient currents along the insulated wire inside precisely similar to those which had been observed when the two functions of inducing magnet and conductor were both discharged by a solid iron wire. The pipe gave much greater currents, chiefly because of its greater size, but partly also because the metal in it was more advantageously disposed than in a solid rod, both as regards the stress and the subsequent inductive effect on the conductor inside.

§ 5. That longitudinal magnetisation combined with torsion should give rise to circular magnetism follows from Sir William Thomson's discovery that æolotropic stress developes an æolotropic difference of magnetic susceptibility in iron. When the wire or tube is twisted the stress is equivalent to pull and push along lines perpendicular to the radius and inclined at 45° to the direction of the length. Along one of these the magnetic susceptibility is increased; along the other, at right angles to that, it is diminished. The effect is to change the lines of induction, originally straight and parallel to the axis, into screws, whose circular components produce the transient currents now described. The direction of the currents is that corresponding to *increase* of magnetism by *pull*.

I shall now give a brief account of the experimental results, and afterwards point out their relation to the discoveries of Villari and Thomson on the effects of stress on magnetism, as well as to certain recent experiments of my own on the same subject.

§ 6. The wire described in § 2 was twisted oppositely to a common screw by turning the pointer through an angle of 60° to the side marked positive (see fig. 1). As the wire was initially free from any considerable magnetisation, this operation gave no transient current. The battery current A was then made, the wire being held twisted. This gave a positive transient current along the wire (from *a* to *b*), which produced a deflection of 39 scale divisions of the ballistic galvanometer. The throw of the galvanometer will be taken as giving, on an arbitrary scale, a measure of the circular magnetisation of the wire.

Then keeping the wire still twisted at +60°, the current A was broken. This gave almost no transient current: in other words, the circular magnetism was scarcely at all affected by the removal of the

longitudinal magnetising force. Other experiments with wires of larger diameter and with a tube have shown that to remove the longitudinal magnetising force when the wire is twisted generally produces a very slight reduction of the circular magnetism, although in some instances it produces an actual increase (see § 22 below). In the present case the breaking of the magnetising current produced no effect at all comparable with that which was produced by its first establishment. Re-making the current after breaking also gave almost no sensible effect.

Reversal from A to B gave a transient current of -78, showing that the circular magnetism changed to -39, and repeated reversals of the current gave the same, or very nearly the same, effect.

§ 7. Again, if instead of twisting the wire first, and magnetising it when twisted, we apply either A or B at the zero of torsion, there is no effect. Let A be kept on, and let the wire be suddenly twisted to $+60°$: there is then a transient current equal to about $+35$. The circular magnetisation acquired in this way is therefore somewhat less than that which was reached by twisting first and magnetising afterwards. If now, at $+60°$ of torsion, we reverse from A to B, we have a transient current of -74: in other words, the circular magnetism is changed from $+35$ to -39. A subsequent reversal from B to A gives $+78$, and so on.

§ 8. If we next keep A made, and change the torsion suddenly from $+60°$ to $-60°$, we get a current, not of -78, but of only about -72. A twist back from $-60°$ to $+60°$ gives a still smaller positive deflection, and successive repetitions of the same operation, under constant magnetising force, cause a gradual diminution of range, until, after many back and forth twistings, the transient currents are -64, when the wire is twisted from $+60°$ to $-60°$, and $+64$, when it is twisted from $-60°$ to $+60°$. This means that the circular magnetism is then changing between $+32$ and -32, on the assumption that the effects are symmetrical on the two sides. That they were very nearly so in the experiment cited I have no doubt; there may, however, have been a slight set towards the side on which the first and greatest circular magnetisation occurred. If, after this steady state has been reached by successive opposite torsions, the battery is reversed from A to B at $+60°$, a transient current of -71 shows that the circular magnetism is at once restored to its normal value of -39.

§ 9. The foregoing results may be stated more clearly and shortly in tabular form, thus:—

Magnetising current.	Torsion.	Transient current.	Circular magnetisation.
Make A.............	Steady at +60°	+39	+39
Reverse A to B......	" " "	−78	−39
" B to A......	" " "	+78	+39
With A on..........	+60° to −60°	−72	−33
" "	After several + and − twists, +60° to −60°	−64	−32
" "	−60° to +60°	+64	+32
Then, Reverse A to B.	at +60°	−71	−39
" B to A.	"	+78	+39

Also, starting from a neutral state—

With A on..........	0° to +60°	+35	+35
Reverse A to B......	at +60°	−74	−39
" B to A......	" "	+78	+39

These figures show that if we call ± 39 the *normal* circular magnetisation proper to the given torsion (60°) and the given longitudinal magnetising force, then the first application or reversal of the magnetising force while the wire is held twisted developes the full normal value of the circular magnetism, whereas the first application or reversal of the torsion under constant magnetising force does not produce the normal value, but something less. Also that this defect of circular magnetism is increased by successive back and forth twistings, which finally cause the circular magnetism to oscillate between two considerably reduced values. After this steady state has been reached, a single reversal of the longitudinal magnetising force, the torsion being kept constant, suffices to produce the full normal value of the circular magnetism proper to the state so arrived at.

§ 10. If after being circularly magnetised by applying longitudinal magnetising force whilst it is held twisted, the wire be relieved of torsion by allowing the twisting arm to return suddenly, but without shock, to zero, a portion of the circular magnetism survives the removal of the stress which gave rise to it. Thus:—

Magnetising current.	Torsion.	Transient current.	Circular magnetisation.
Make A........	At +60°	+39	+39
With A on.....	+60° to 0°	−24	+15
Also, " "	0 to −60°	−48	−33

From the last observation it will be seen that the second step

(0° to −60°) brings us to the value −33, almost identical with that which was reached in § 9 by twisting at once from +60° to −60°.

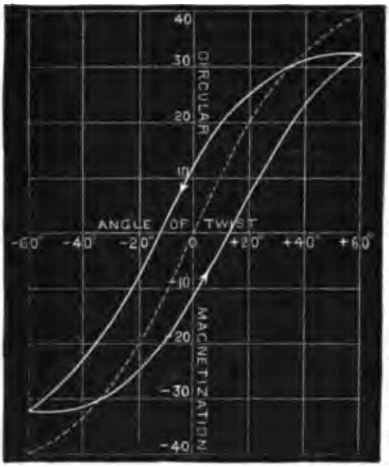
This tendency on the part of the circular magnetisation to follow at a distance instead of accompanying the changes of torsion is still more clearly shown if we divide the whole angle from +60° to −60° into several steps. The following figures give the transient current so obtained after the torsion had been reversed sufficiently often to bring the changes into a steady and sensibly cyclic state. The current A was kept on throughout.

Torsion.	Transient current.	Circular magnetisation.
−60° to +60°	+64	+32
+60 „ +30	− 3	+29
+30 „ 0	−17	−12
0 „ −30	−28	−16
−30 „ −60	−16	−32

with similar values for the return stages.

The full lines in fig. 2 give the relation of circular magnetisation to angle of twist during this cyclic operation. They show well how the changes of the former lag behind those of the latter.

Fig. 2.



§ 11. The same tendency towards persistence of previous state is exhibited whenever we change the magnetisation of a piece of iron or steel by the alternate application and removal of any kind of stress. It is exhibited also when magnetisation is changed by changing the magnetising force, when it appears as the characteristic ordinarily called retentiveness, to which the existence of residual magnetism is

ascribed. It also appears, as was shown first by M. E. Cohn, and afterwards independently by myself in a paper which I had the honour recently to lay before the Royal Society,* in the changes of thermoelectric quality which occur in iron when it is subjected to cyclic changes of stress. In the instances now referred to this lagging seems to be permanent as regards time, so that if it is to be ascribed to molecular friction, the friction to which it is due must resemble the friction of solids rather than the viscosity of liquids. To avoid much circumlocution it is convenient to give this lagging action a name, and accordingly I have called it Hysterēsis (from ὑστερέω, to come after, used either of place or time). This name may be properly applied not only in cases where, as here, the persistence of previous state appears to be permanent as respects time, but also in those cases where (as in the relation of strain to stress in a viscous material) the amount of lagging depends on the rate of change of the conditioning quality, and would disappear if that were indefinitely slow.

To define the new term more precisely, let there be given two qualities of matter, M and N, of which M is a function of N; then if when N is changed cyclically the corresponding changes of M lag behind the changes of N, we may say that there is "hysteresis" in the relation of M to N. The value of M at any particular point of the operation depends not only on the actual value of N but on all the preceding changes of that quantity, and by properly manipulating those changes any value of M within more or less wide limits may be found associated with a given value of N.

In all the instances of "hysteresis" mentioned above this further characteristic is present, that the range through which M varies becomes gradually diminished when a cyclic variation of N is repeated several times, and it would seem that only after an indefinitely large number of repetitions of the cycle of N do the changes of M also become exactly cyclic.†

§ 12. In the paper referred to above I showed that the action here called hysteresis, when it exhibited itself in the relation of thermoelectric quality to stress in a soft-iron wire, could be nearly, if not wholly, destroyed by mechanically agitating the wire during or after

* "Proceedings," vol. 32, p. 399.

† Objection may, perhaps, be taken to the coining of a new word on the ground that the term "retentiveness," already in use, expresses sufficiently nearly the same idea. In physics, however, retentiveness is limited by custom to denote the power of retaining magnetism when the magnetising force is removed, whereas one of the instances in which "hysteresis" has been noticed has, at least apparently, no connexion with magnetism. And unless the word retentiveness be used in a sense much less restricted than is now customary, it will not cover even those cases of "hysteresis" which occur in magnetic phenomena.

the change of stress. The same thing is true in the present case. After the circular magnetisation has been reduced from 39 to 32 by successive twistings between +60° and −60° under constant magnetising force, it can be at once raised again to 39 by tapping the wire vigorously. Again, if in making the steps +60° to 0° and 0° to −60° under constant magnetising force, we permit the wire to spring back freely to the zero of torsion and oscillate there (instead of making the twisting arm come against a stop which arrests vibration), we find that the two steps give nearly equal transient currents—in other words, that at the zero of torsion there is then almost no circular magnetisation.

§ 13. A still more effective way of restoring the circular magnetisation to its normal value after it has been affected by hysteresis is to change suddenly the longitudinal magnetisation of the wire. The molecular agitation so produced acts like mechanical vibration. Almost all trace of the effects of hysteresis vanishes if we simply break and remake the magnetising current in the solenoid. This is especially true of soft iron; with steel the effects of hysteresis are only partially removed by this means.

For example, let the circular magnetism be reduced to 32 by successive twistings between +60° and −60° under the A current, then if we break A and remake it, each of these operations gives a small positive transient current, the two together showing that the circular magnetism has risen to very nearly its full value of 39. Or, again, if after applying A at +60° we untwist to zero, keeping A on, there is a residual circular magnetism of +15: then let A be broken and remade, and each of these operations will give a negative transient current, the two together amounting as nearly as possible to −15, after which subsequent makes and breaks of the magnetising current will give no effect. If on the other hand we reach the zero of torsion from −60° with A on, the residual circular magnetism is negative, and in that case positive transient currents are produced when A is broken and remade. Generally, to remove and reapply, or better, to reverse the longitudinal magnetising force while there is no torsion, has the effect of removing any circular magnetisation which may have remained from previous operations.

§ 14. To determine the normal value of the circular magnetisation corresponding to different angles of twist, with the same longitudinal magnetising force, we have simply to bring the twisting arm to any angle, and, holding it there, reverse the magnetising current more than once. The first reversal wipes out the effect of previous operations and sets up the normal state of circular magnetisation; the second reversal changes that from + to −, and so gives a transient current half of which is to be taken as the measure of the circular magnetisation. The following observations were made at different

angles of twist from 10° to 60°, the currents being observed after at least one previous reversal of the magnetising force.

Angle of twist (scale reading).	Transient current given on reversal of battery.		Circular magnetisation.
	A to B	B to A	$= \frac{\text{mean}}{2}$
+10°	−25	+25	12½
−10	+21	−21	10½
+20	−41	+43	21
−20	+42	−38	20
+30	−55	+56	28
−30	+54	−52	26½
+40	−67	+67	33½
−40	+66	−64	32½
+50	−73	+75	37
−50	+71	−72	36
+60	−78	+80	39½
−60	+76	−78	38½

The same results are shown graphically in the dotted line of fig. 2; comparison of it with the full lines of the same figure will serve to show the part played by hysteresis in the changes of circular magnetisation which are caused by changes of stress.

§ 15. The torsions which have been hitherto spoken of were well within the limits of elasticity. When the angle of twist was increased beyond 60° the transient currents given on reversal of the battery were not greatly augmented. They reached a maximum at about 90°, at which angle the torsion began to produce distinct permanent set, and for greater angles they diminished slightly. Moreover, after the wire was twisted beyond its elastic limit, and allowed to come back to its new zero of torsional stress, reversal of the longitudinal magnetism then gave transient currents of opposite sign to those which were given while the stress which caused the set was in operation; the reason probably being that the wire, though then free from torsional stress, had acquired a helical quality with respect to magnetic inductive susceptibility, the susceptibility being less along the lines of permanent extension than in other directions. The lines of induction were therefore screws of opposite sign to the lines of permanent extension. Again, effects of the same sign as those observed would be given by permanent twist if permanent extension produced a state in which the electrical conductivity of the iron was less along than across the lines of strain. After being twisted to +360° the wire was allowed to spring back: its new zero of stress was at +125°. There, after several reversals, B to A gave −7, and

A to B gave +7. By restoring 5° of positive torsion a state was reached in which reversals of the battery gave no transient currents.

§ 16. The effects which are obtained by twisting and untwisting after the longitudinal magnetising force is removed are somewhat more complex than those already described, on account of the fact that a gradual shaking out of the residual magnetism takes place, which is not, however, complete even after many twistings. For example:—

	Transient current.		Circular magnetisation.
At +60°, reverse A to B.	+76	......	+38
Break A	sensibly 0	......	+38
+60° to 0°..........	−18	......	+20
0 „ −60	−19	......	+ 1
−60 „ 0	+ 4	......	+ 5
0 „ +60	+12	......	+17
+60 „ 0	− 4	......	+13
0 „ −60	− 8	......	+ 5
−60 „ 0	+ 4	......	+ 9
0 „ +60	+ 8	......	+17
+60 „ −60	−11	......	+ 6
−60 „ +60	+11	......	+17

Ten more complete wistings; then

+60° to −60°..........	− 9	......	+16
−60 „ +60	+ 8	......	+ 8

It is interesting to notice that the circular magnetisation has, so to speak, received a permanent set to the side on which its earliest and greatest value lay.

§ 17. Another experiment, to show the production of circular out of residual longitudinal magnetism:—

	Transient current.		Circular magnetisation.
Make and break A at 0° ...	0	......	0
Then, 0° to +60°.........	+18	......	+18
At +60, remake A........	+20	......	+38

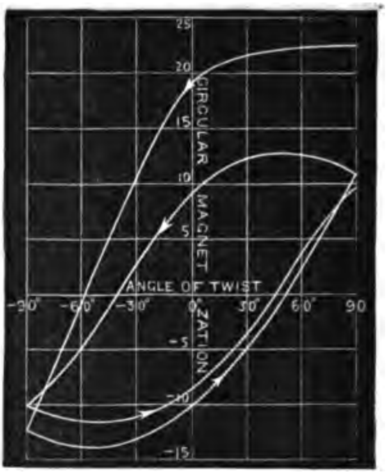
§ 18. The following table shows the relative amounts of circular magnetism developed in an iron wire (the wire of § 2), with one constant amount of torsion (60°) by different intensities of longitudinal magnetising force. It will be noticed that a maximum of effect is passed at about 15 or 16 c.g.s. units.

Longitudinal magnetising force. c.g.s. units.	Transient currents given on reversal of magnetising current.		
	A to B	B to A	Mean.
1·25	− 2	+ 2	2
1·64	− 3	+ 3	3
2·36	− 7	+ 7	7
3·20	−15	+15	15
3·64	−21	+21	21
4·2	−33	+32	32·5
5·0	−46	+45	45·5
6·1	−59	+57	58
8·0	−71	+70	70·5
11·3	−82	+80	81
14·0	−86	+84	85
15·8	−86	+85	85½
17·1	−86	+84	85
19·7	−84	+83	83·5
23·7	−75	+75	75

A continuation of the same experiment showed that when the magnetising force was increased the transient currents continued to diminish, falling almost to zero, but did not become reversed. Even with a wire of very soft iron a magnetising force of over 100 c.g.s. units still gave effects of the same sign as the above.

§ 19. Similar series of experiments were made with a piece of pianoforte-steel wire in its ordinary temper. Fig. 3 shows how the

Fig. 3.



circular magnetisation of the steel wire, starting from the norma value for +90°, was altered by successive twistings between +90° and −90° under the action of a constant longitudinal magnetising force of 20 c.g.s. units. It exhibits very strikingly the lessening of range brought about by successive twistings, which has been already alluded to (§ 8) as occurring (though to a less extent) in iron. Finally, after several twistings, a sensibly cyclic set of changes was instituted which is shown by the full lines in fig. 4. [The "normal" curve

Fig. 4.

(see § 9) is shown by the dotted line in the same figure.] A remarkable feature here is that when the direction of twisting is reversed the first effect is to continue the same kind of change of circular magnetism as was going on before. The curve rises before it begins to fall, and *vice versâ.*

Many other observations have been made, which it is needless to describe, since they are all in obvious agreement with the explanation already offered. It only remains to point out this agreement in the case of those features of the experiments where it is not at once evident.

§ 20. It was discovered by Villari,* and rediscovered by Sir W. Thomson,† that if an iron rod in a magnetic field be subjected to pull

* Wiedemann's "Galvanismus," II, § 499.

† "Phil. Trans.," Part I, 1879, p. 55.

parallel to the lines of force its magnetism is increased, provided that the magnetising force does not exceed a certain value, but decreased if the magnetising force does exceed that limit. Experiments of my own, not yet published, have shown that the Villari reversal of the effect of pull occurs at a particular value of the magnetisation rather than at a particular value of the magnetising force. They have also shown that, apart from its influence on magnetic susceptibility, pull increases residual magnetism when that is not too strong (this Villari had observed), and further, that the changes of either induced or residual magnetism produced by changes of stress always exhibit the action I have called hysteresis, with the characteristic described in § 11, viz., diminution of range in successive operations, with an approach to a cyclic condition when a cyclic change of stress is repeated. The lagging of magnetic change is just such as might, on Weber's theory of magnetic induction, be ascribed to a frictional resistance opposing the rotation of the magnetic molecules; further, it is capable of being, in great part, destroyed by mechanical vibration.

The experiments now under review exhibit the same actions occurring when we deal with the combined pull and push stress set up by torsion.

§ 21. In view of the discovery of Villari and Thomson I expected that with a high magnetising force the signs of the transient currents would be reversed, so that they would then indicate decrease of magnetism by pull. This did not occur (§ 18) apparently for the following reason. My own direct experiments show that moderate* pull increases the magnetism of iron only so long as that is less than (very roughly) about three-fourths of its limiting value. Now, when a longitudinally magnetised rod is twisted, the pull and push of which the stress is made up act not on the full magnetisation but on components of it inclined at 45° to the axis. The intensity of magnetism on which the stress acts is therefore rather less than three-fourths of the whole, and hence, even when we increase the magnetising force indefinitely, the action still occurs below, though very near to, the Villari critical point. Under these circumstances the transient currents become much diminished in amount, but they preserve the same signs as they have when the magnetising force is small, the signs, namely, which correspond to increase of magnetism by pull.†

* The precise point of reversal depends, amongst other things, on the amount of pull applied.

† Sir W. Thomson (*loc. cit.*, § 229) has applied his discovery of the effects of stress on the magnetic inductive susceptibility of iron to explain G. Wiedemann's observation that an iron rod traversed longitudinally by an electric current becomes a magnet when twisted, and expresses his surprise at finding no reversal of the poles when a strong current was used; but if the above explanation of the non-reversal

§ 22. It has been mentioned that the removal and re-application of longitudinal magnetising force acting on an iron wire under torsion produced almost no change of the circular magnetism, and that when a small change was visible, it had sometimes the apparently anomalous character of increase of circular magnetism with removal of longitudinal magnetising force. My experiments show that the residual magnetism of soft annealed iron is generally very great, amounting to as much as 80 or even 90 per cent. of the induced magnetism, provided that care be taken, by the use of a very long rod or a ring magnet, to prevent the existence of any self-demagnetising force when the inducing force is withdrawn (a condition present in these torsion experiments), and also provided that mechanical disturbance be avoided, which, if it occurs, will remove by far the greater part of this large residue. Now any change of circular magnetism which takes place when the longitudinal magnetising force is withdrawn from a twisted wire must be due to the fact that the effect of torsion in producing circular magnetism is either less or greater on the residual than on the total longitudinal magnetisation. The two are so nearly equal that but little change occurs, and in general, when the longitudinal magnetising force has not been very intense, the change is of the nature of a diminution. But when the longitudinal magnetisation has been very strong, the 45° components of it, on which the effects of pull and push are felt, may approach so closely to the Villari critical point that the reduction of them which takes place when the magnetising force is removed makes them more susceptible to the action of stress, and so causes an increase of circular magnetism.

§ 23. To complete the explanation of the results, allusion need be made to only one other point. It was mentioned in § 19 that when by successive twistings from one side to the other, a cyclic condition was established in steel, then at the beginning of each release from torsional stress a slight increase of circular magnetism took place (see fig. 4). My experiments on the effects of stress on magnetised wires show that at the beginning of each loading or unloading the initial change of magnetism is *nil* relatively to the initial change of stress, if great care be taken to avoid mechanical disturbance. When there is any disturbance at the beginning of the change of load, its effect is to make the magnetisation approach the value which it would assume under vibration. In the torsion experiments the changes of stress were necessarily effected suddenly, and must have been accompanied by some slight vibration. This affords a sufficient explanation of the

of the transient currents be correct, it applies equally to Wiedemann's result. There, too, the intensity of magnetisation on which the differential effect of pull and push is felt, being $\frac{1}{\sqrt{2}}$ of the whole circular magnetism, cannot be increased sufficiently to cause the Villari reversal.

point now under consideration; for in the observation shown in fig. 4, the initial effect of beginning to release the wire from stress, instead of being *nil*, was in fact just that which a small amount of vibration would cause.

§ 24. The fact that sudden torsion gives a transient current along a magnetised iron or steel rod was observed by Matteucci as early as 1858. An account of his observations is given by Wiedemann ("Galvanismus," II, § 484), from which it appears that when an iron rod under the influence of longitudinal magnetising force was twisted like a common screw, the current flowed from south to north. The direction stated is opposite to that taken, under similar conditions, by the currents in my experiments. A possible explanation of this discrepancy may be found if we suppose that in Matteucci's experiments the Villari critical point had been passed, which might have been the case if he applied strong torsional stress in conjunction with strong magnetisation. For it has been shown by Sir W. Thomson,* and confirmed by my own experiments, that the Villari critical point comes earlier with strong than with weak stresses. With the moderate stress used by me, reversal of the effects did not occur even with very high magnetising forces, but it is possible that by using more powerful torsion Matteucci may have brought his rods into the condition which would give decrease of magnetism by oblique pull. This suggestion is borne out by his observations with hard steel, of which it is said that when the magnetising current was broken, the transient currents produced by torsion changed their signs after the residual magnetism had been partly shaken out by the first twistings, showing apparently a passage through the Villari critical point as the magnetisation was reduced.

Matteucci has also remarked that the currents become constant only after several back and forth twistings. Curiously enough he says that a twisted rod gives no current when it is magnetised, but it is not unlikely that this (certainly erroneous) observation may have been due to an accidental omission to notice the effect of the first closing of the magnetising circuit after the rod was twisted. Subsequent openings and closings of the circuit do give scarcely any effect.

§ 25. By using a telephone in place of a ballistic galvanometer, Professor Hughes has observed the production of transient currents in a twisted iron wire by making and breaking a current in a surrounding solenoid, and he has described in a recent series of papers† this as well as many other closely related results. For example, he placed the iron wire in the battery circuit, and connected the telephone, to the external solenoid. Sounds were then obtained

* *Loc. cit.*, §§ 211 and 244. † "Proc. Roy. Soc.," vol. 31.

by rapidly interrupting the current through the wire. This (which I have verified with a ballistic galvanometer) is a direct consequence of Wiedemann's discovery alluded to in a footnote to § 21 above.

§ 26. Apart altogether from the magnetic origin to which the transient currents described in this paper have been assigned, another cause probably contributes in some small degree to the production of the observed results. From Sir William Thomson's discovery of the effects of stress on electrical conductivity it follows that an originally isotropic conductor will, when under torsion, possess a helical quality with respect to electrical conductivity, and it has in fact been shown experimentally by Professor W. G. Adams and Mr. J. T. Bottomley that a brass tube, conveying a current longitudinally, becomes when twisted equivalent to a conducting helix. Hence when the longitudinal magnetism of a twisted iron rod (forming part of a circuit) is altered, a transient current will be induced in the helical lines of greatest conductivity by the magnetic change of the interior portion of the rod. The direction of greatest resistance in iron is (probably) that of pull, and the lines of greatest conductivity will therefore be helices opposite in sign to the twist of the rod. Magnetisation will therefore give a current from S. to N. when the twist is that of a common screw. The actual transient current, however, flows from N. to S. Moreover it is clear that the development of a helical quality with regard to conductivity will not explain the fact that the transient currents pass a maximum when the current in the solenoid is strengthened, nor apply at all to the gas-pipe experiment of § 4. It has been suggested to me that another more recondite partial origin of the transient currents given when a magnetised iron rod is twisted may be looked for in the fact that the diminution of longitudinal magnetism brought about by torsion induces a current in the magnetising solenoid, which again reacts on the helical lines of conductivity in the rod itself,—an idea which was perhaps present with Professor Hughes when he spoke of the transient currents as "tertiary." It is safe to say that these and perhaps other influences enter into the production of the effects which have formed the subject of this paper; at the same time it may be affirmed with confidence that the phenomena, as they are observed, find a perfectly satisfactory and sufficient explanation in the setting up of a state of circular magnetisation by the influence of torsional stress on the existing longitudinal magnetism and on the susceptibility to magnetic induction.

Supplementary Note to the Original Paper. Received August 30, 1882.

In the original paper under this title, communicated to the Royal Society on September 7th, 1881, it was suggested that the phenomena were due to the production of a state of circular magnetisation by the action of twisting stress on longitudinal magnetisation, the effect of twist being to produce a difference of magnetic susceptibility in the two directions of pull and push, inclined at 45° to the axis. Recent experiments of my own on the effect which pull has on the magnetic susceptibility and residual magnetism of iron (an account of which will be given separately) confirmed this conjecture; and, guided by the light which they threw on the subject, I have now made a short supplementary examination of the effects of torsion, which has shown that the conjectural explanation is perfectly satisfactory.

To show that the transient currents produced by twisting a magnet are due to the development of circular magnetisation, I substituted for the iron or steel wire used in former experiments an iron gas-pipe, itself insulated but carrying along its interior an insulated copper wire which was in circuit with a ballistic galvanometer. The gas-pipe was longitudinally magnetised by a surrounding solenoid. When the pipe was twisted, a transient current passed along the wire in its interior. Another transient current was given when the longitudinal magnetisation was reversed, the state of twist remaining constant. By this arrangement, in brief, all those phenomena could be reproduced which were described in the paper as exhibited when the two functions of inducing magnet and conductor were both discharged by a solid iron wire. The transient currents given by the tube were much more powerful, partly because the position of the conductor was more advantageous than when conduction was taking place throughout the twisted metal itself, but chiefly because of the relatively large size of the tube. By winding the insulated wire in the centre so that it passed several times through the tube, the effects were proportionately increased.

In the paper it was shown that when the longitudinal magnetising force was increased, the transient currents given by reversing that force while the wire was kept in a constant state of twist passed a maximum when (with one specimen) the value of the force was 15 c.g.s. units, after which the effects diminished slowly as the force was raised to 24 c.g.s. units, that being the highest value then used. The effects were such as to correspond to *increase* of magnetism along the lines of pull. From the discovery of Villari and Thomson that with a certain value of the magnetising force the effect of pull on magnetisation becomes reversed, we might expect the signs of the

transient currents to change if the magnetising force were sufficiently increased. Recently I have examined the effects with very high values of the longitudinal magnetising force. The transient currents are then exceedingly small, but they obstinately refuse to become reversed, even when the magnetising force is as much as 100 c.g.s. units. The effects still correspond to greater magnetisation along the lines of pull.

The explanation of this seeming anomaly lies in the fact that the Villari reversal of the effects of stress depends on the intensity of magnetisation rather than on the value of the magnetising force, and that the stress acts here only on a component of the whole magnetisation. I find that in soft iron, the effect of a moderate amount of pull is to give greater susceptibility so long as the magnetisation does not exceed about three-fourths of its limiting value. With a greater stress the reversal comes earlier, and with a very small stress it comes later. Now, suppose that we apply to a soft-iron wire a powerful magnetising force and thereby approach the limit of magnetisation. When we twist the wire, the directions of pull and push are inclined at 45° to the direction of magnetisation, and the stress therefore acts on $\frac{1}{\sqrt{2}}$, or about seven-tenths of the whole intensity of magnetisation. Hence pull ought still to increase (though very slightly) the magnetic susceptibility along the lines of pull, and the circular magnetisation ought still to have the same direction as it had when the magnetising force was small; and this is what actually occurs.

In the paper, it was noticed that when the magnetising current was interrupted while the wire was held in a constant state of twist, there was scarcely any change of the circular magnetisation. I now find that although in general the circular magnetisation decreases very slightly in these conditions, nevertheless it occasionally increases when the longitudinal magnetising force is removed. This happens in soft iron when that force is very high, and its occurrence is in complete agreement with direct observations of the effects of pull on magnetism. It means that the effect of pull is then greater on the 45° component of the residual magnetism than on the same component of the temporary magnetism, and this is the case when the affected component of the temporary magnetism is near that value at which the Villari reversal occurs. The following figures give an observed instance of this action in a very strongly magnetised soft-iron wire which was held in a constant state of twist. Calling the two directions of the magnetising current A and B, and the two directions of circular magnetisation + and −, we have:

	Ballistic throw.		Circular magnetisation.
Reversed, A to B.....	+ 8	....	+ 4
" B " A.....	− 8	....	− 4
Broke A.............	− 9	....	−13
Made B.............	+17	....	+ 4
Broke B.............	+ 9	....	+13
Made A.............	−17	....	− 4

Here the circular magnetisation was greater in the ratio of 13 to 4 when the differential effect of torsional stress was exerted on the (comparatively small) amount of residual than on the (comparatively large) amount of temporary longitudinal magnetism.

December 6, 1883.

THE PRESIDENT in the Chair.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
The Duke of Argyll.
Mr. De La Rue.
Mr. Francis Galton.
Professor Prestwich.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Description of Parts of a Human Skeleton from a Pleistocene (Palæolithic) Bed, Tilbury, Essex." By Professor OWEN, C.B., F.R.S., &c. Received November 26, 1883.

(Abstract.)

The subject of the present paper was discovered during the excavations of docks now in progress at Tilbury Fort, Essex, at a depth of 32 feet from the present surface. It consists of a considerable proportion of a human skeleton, the parts of which are determined and described. The inferences deduced are that they were from a somewhat aged male of great muscular strength; and such inferences as to food, as might be drawn from the worn crowns of the teeth in use at his demise, are given. A chemical analysis of the bones is added by Dr. Walter Flight, of the Laboratory Department in the British Museum. A section of the several strata dug through before arriving at the bed is appended.

This section determines the man to have lived at the so-called "Palæolithic period." The author acknowledges his indebtedness to Colonel Du Plat Taylor for the transmission of the skeleton, with a notification of its discovery, in a letter of the 1st October, 1883; also to Mr. Donald Baynes, engineer of the dock works, who transmitted a section of the strata. These consist, from the grave-bed upwards, of "sand," "mud," "peat," "mud and peat," "mud," "clay."

Figures of the bones and teeth described, and "plan of the section," accompany the text.

II. "The Wave-lengths of A, *a*, and of Prominent Lines in the Infra-Red of the Solar Spectrum." By Captain W. DE W. ABNEY, R.E., F.R.S. Received November 20, 1883.

M. Fievez has recently sent me a map of the solar spectrum from C to A* inclusive, and as part of this region is one which I have been measuring, I have examined the new publication with great interest. Photography and eye measurements do not exactly coincide in the detail of the grouping of the little *a* group as far as A, and A itself is shown by M. Fievez's map as wanting some details which appear in the photographs. Thus in the photographs there are some seventeen lines, whilst in M. Fievez's map there are but thirteen. Between A and *a* there are several lines of marked intensity in the photograph which are not shown in the new map. The wave-lengths of the different lines from above "*a*" to A are not the same as those given by Fievez, when they are taken from comparison photographs of the 1st order of the red and 2nd of the ultra-violet on the same plate, or when checked by photographs of the 2nd order of the red with the 3rd order of the green taken in a similar manner. In my paper, "Phil. Trans.," Part II, 1880, I gave a method of using mirrors by which this could be effected, but since Professor Rowland introduced his concave gratings this is much more readily carried out. He has kindly furnished me with gratings for the purpose, having about 14,400 lines to the inch, with focal distances of 7 feet 6 inches and 12 feet 6 inches respectively. These have been employed in determining the wave-lengths of this part of the spectrum. Cornu's map was used as a reference for the ultra-violet wave-lengths, and Ångström's map for those in the blue and green. The two maps may be taken as equally exact.

Description of line.	λ from comparison of 1st and 2nd orders.	λ from comparison of 2nd and 3rd orders.	λ according to Fievez.	Remarks.
"a"......	7184·4 7185·4	7184·5 7185·4	7197·7 7198 7	This is shown in Ångström's map as a single line λ 7184·9.
Most refrangible edge of A.	7593·6	7593·7	7600·0	Ångström gives 7604 for the centre of this line, which of the bands he took as A is not clear. Langley gave 7600·9 for this edge.

* "Annales de l'Observatoire Royal de Bruxelles," nouvelle série, tome V.

Description of line.	λ from comparison of 1st and 2nd orders.	λ from comparison of 2nd and 3rd orders.	λ according to Fievez.	Remarks.
Centre of 6th pair of lines in the flutings following A.	7643·2	7643·33	7652·2	

The determination of A has been made by Mascart, Smyth, and others, besides Ångström and Langley, with discordant results. I think the above may be taken as accurate as are Cornu's and Ångström's maps.

It may be useful to forestal the detailed publication of my measurements by giving the wave-lengths of a few of the principal lines in the infra-red. The scale numbers refer to my map of the infra-red, which is published in the "Phil. Trans.," Part II, 1880.

Scale number.	Description.	Wave-lengths.
1046	This line is a triple line, the two strongest components of which have the accompanying wave-lengths....................	8226·4, 8229·9
1441	....	8496·8
1509	....	8540·6
1685	....	8661·0
2175	A double line, the components of which have the accompanying wave-lengths...........	8986·2, 8989·5
2638	" " "	9494·5, 9500·1
3161	....	9633·8

The measurements of the lines have been made with a micrometer by Hilger. The $\frac{1}{10000}$ of an inch can be easily measured, and in extreme cases the $\frac{1}{100000}$ of an inch can be recognised.

December 13, 1883.

THE PRESIDENT in the Chair.

The Right Hon. Lord Justice Sir Edward Fry, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on a Series of Barometrical Disturbances which passed over Europe between the 27th and the 31st of August, 1883." By ROBERT H. SCOTT, F.R.S., Secretary to the Meteorological Council. Communicated by desire of the Council. Received December 4, 1883.

Plate 1.

The occurrence of sudden temporary derangements of atmospheric pressure has been occasionally noticed at all observatories provided with barographs, either mechanical or photographic.

Among the most remarkable of these which have been recorded in these islands have been that of January 16, 1869, which appeared at Aberdeen, and to a less extent at Glasgow, and that of January 30, 1876, which was noticed chiefly at Armagh and Aberdeen, and was described by me in a note published by the Meteorological Society ("Quarterly Journal," vol. iv, p. 73).

In both of these cases the depression of the barometrical column amounted to about 0·1 inch, and the duration of the entire disturbance to about ten minutes. In both cases the anemometers showed sudden disturbances both in the direction and force of the wind, and on the latter occasion certainly a shower fell. In 1869 no self-recording rain-gauges existed.

It is evident that both of these disturbances were due to the passage of squalls.

The phenomena which I have now to notice are very remarkable, inasmuch as they are not accompanied by any traceable disturbance of any other element than pressure, and they appear as clearly at Coimbra and St. Petersburg as at our own observatories.

The broad facts to be recorded are that at the end of August a violent volcanic eruption took place in the Straits of Sunda (in 105°

east longitude). The continuance of the shocks is given at from 4 P.M. on the 26th to daybreak on the 27th, corresponding to the interval from 9 A.M. to 10 P.M. on the 26th, Greenwich time; but they probably continued for a longer period. No precise statement as to the moment of occurrence of any particular explosion or shock has as yet been printed.

Two letters have been received at the Meteorological Office from the Board of Trade, one from Her Majesty's Consul at Batavia, and the other an extract from the log of the Dutch steam ship "Governor-General Loudon," which ship was in Sunda Straits at the time of the eruption, having called at Anjer the day before it took place, and again after the place had been swept by the earthquake sea-wave. Neither of these accounts contains any precise statement as to time of any particular phenomenon.

The facts which I have to bring to the notice of the Society are the indications of successive disturbances of the barometer occurring also at the end of August, at regular intervals and at every observatory in Europe.

I shall distinguish the four disturbances shown on the engraving by Roman numerals. Greenwich time is used.

I. At about 11 h. A.M. on the 27th, a sudden increase of pressure, followed by a decrease, appeared at St. Petersburg, and a similar phenomenon was noticed at Valencia Island, and at Coimbra in Portugal, as well as at all the intermediate observatories over Europe from which we have been able to obtain tracings of barograms.

The character of the disturbance was not strictly identical, for at the western stations the rise of the barometer was more marked than at the eastern. The general appearance of the barograms at adjacent stations is strikingly similar. It is, however, difficult to select any peculiarly remarkable phase of the disturbance so as to recognise it and record the time of its occurrence at each observatory.

This movement (I) was propagated from east to west at a very high velocity, for the recovery of pressure from the first decrease occurred at St. Petersburg at noon, and at Valencia at 2 h. 25 m. P.M. on the same day, thus taking only two hours and twenty-five minutes to traverse the distance of 1,315 miles between the two observatories.

II. A somewhat similar disturbance appeared on the 28th, but was propagated from west to east, reaching Valencia at 3 h. 20 m. A.M., and St. Petersburg at 5 h. 15 m. A.M., and thus requiring only one hour and fifty-five minutes for its passage. The same uncertainty as to identification of the phase exists in this case as in the preceding. In all cases, however, the most marked phase of the phenomenon has been noted.

III. A disturbance travelling in the same general direction as No. 1,

but from E.S.E. to W.N.W., reaching St. Petersburg at 0 h. 20 m. A.M. on the 29th, and Valencia at 2 h. 28 m. A.M. on the same day, and traversing the distance in two hours and eight minutes.

IV. A disturbance travelling in the same general direction as No. II, but from W.N.W. to E.S.E., reaching Valencia at 2 h. 0 m. P.M. on the 29th; and St. Petersburg at 3 h. 35 m. P.M., and occupying only one hour and twenty-five minutes in passage.

Similar disturbances, though of a gradually diminishing intensity, can be traced in most of the barograms, occurring at Valencia at about 3 h. P.M. on the 30th and 2 h. A.M. on the 31st. After this time the traces of disturbance become less distinctly recognisable. Some of the oscillations are more marked at some stations than at others; the Scotch observatories, in particular, exhibit the later disturbances very distinctly.

The engraving, which has been prepared in the Meteorological Office, and which shows all the records reduced to the same scale and to Greenwich time, exhibits the barograms at the following stations, which are enumerated in order of longitude, going from east to west:—

St. Petersburg.
Pawlowsk.*
Vienna.*
Brussels.
Paris.
Geldeston (near Beccles, Norfolk).
Greenwich.
Kew.
Oxford.
Aberdeen.
Stonyhurst.
Liverpool.
Glasgow.
Falmouth.
Armagh.
Coimbra.
Lisbon.*
Sierra da Estrella.*
Valencia.
Toronto.

The stations marked with * have not been engraved yet. A table is appended showing the precise times of occurrence of the different phases of the phenomena at each station as accurately as we can determine them.

I may conclude by saying that the actual record on the barograms

Times of Commencement of the Barometrical Oscillations at the under-mentioned Stations, August 27th, 28th, and 29th, 1883.

Stations.	(1st.) August 27th.			(2nd.) August 28th.		(3rd.) Aug. 29th.		(4th.) Aug. 29th.		Amplitude of the Oscillation.			
	P.M.	P.M.	P.M.	A.M.	A.M.	A.M.	A.M.	P.M.	P.M.	(1st.) inch.	(2nd.) inch.	(3rd.) inch.	(4th.) inch.
St. Petersburg	11 5 (A.M.)	11 35 (A.M.)	0 0	4 40	5 15	0 20	1 0	3	3 35	0·040	0·024	0·012	0·034
Brussels	0 33	0 58	1 30	3 48	4 28	1 43	2 8	2 23	2 43	·051	·039	·039	·017
Geldeston	0 42	1 12	1 40	3 10	4 7	1 40	2 2	2 14	2 44	·060	·040	·035	·010
Kew	0 50	1 10	1 47	3 10	4 5	1 45	2 13	2 0	2 43	·049	·051	·024	·011
Oxford	0 53	1 23	1 50	3 0	3 57	1 55	2 15	1 53	2 40	·030	·030	·015	·015
Aberdeen	1 0	1 25	1 53	3 10	3 52	1 50	2 33	1 53	2 30	·062	·052	·046	·036
Stonyhurst	1 0	1 28	1 55	2 57	3 45	2 0	2 27	1 48	2 33	·062	·064	·029	·050
Glasgow	0 58	1 36	2 5	3 0	3 45	2 10	2 37	1 42	2 27	·046	·064	·060	·028
Falmouth	1 7	1 35	2 5	2 57	3 45	2 5	2 25	1 20	2 28	·050	·080	·030	·030
Armagh	1 13	1 45	2 12	2 53	3 33	2 20	2 45	1 46	2 22	·049	·062	·029	·036
Valencia	1 20	1 48	2 25	2 30	3 20	2 28	2 50	1 27	2 10	·060	·070	·040	·040
Coimbra	..	1 50	2 20	3 5	3 15	2 10	2 15	2 10	2 35	0·066	0·043	0·043	0·043

NOTE.—The figures in antique type indicate a *rise* of the barometer, those in ordinary type a *fall*.

exhibits considerable similarity to that of the King's barograph at the Liverpool Observatory, at the Waterloo Docks Pierhead on the 15th of January, 1864, when the "Lottie Sleigh," loaded with about twelve tons of gunpowder, blew up. The ship was lying about three miles from the Observatory. I am indebted to the kindness of Mr. Hartnup for a tracing of this interesting curve.

In this preliminary note I can only say that until copies of the barograms from extra-European stations, such as New Zealand, the Havaña, and Calcutta, are received, it is impossible to see whether the phenomena so marked in their occurrence in Europe have manifested themselves elsewhere; at Toronto they appear to have been recorded. On that subject I shall hope to lay a future communication before the Society.

II. Note on the foregoing Paper. By Lieutenant-General R. Strachey, F.R.S. Received December 12, 1883.

The obvious correspondence of the forms and times of occurrence of the barometric disturbances, described in Mr. Scott's paper, at once suggested that they were due to a common origin, and the great volcanic eruption at Krakatoa in the Straits of Sunda appeared to supply a probable efficient cause. Any shock of sufficient violence might be expected to produce an atmospheric wave, advancing from the place where it was caused in a circular form round the globe, at first expanding until it had got half round the earth, and then again contracting till it was again concentrated at the antipodes, from which again it would be thrown back, and so pass backwards and forwards till it was obliterated. It might also have been expected that such a wave would travel with the velocity of sound, being probably of the same nature as that which causes sound, though the vibrations had not the peculiar character that affects our organs of hearing. It has, however, been suggested to me that the wave may rather have had the character of a solitary wave produced in a liquid, the velocity of which in the air would not materially differ from that of sound.

A rough examination of the facts at first made known by the observations recorded in Great Britain indicated that there was *primâ facie* strong evidence in support of this view, and that the phenomena would be approximately explained by the passage round the earth of a series of waves travelling at the rate of about 700 miles an hour in opposite directions from the place where the volcanic eruption occurred. The records since procured from other places, and the more careful examination of the facts, has quite confirmed this conclusion.

Although we may expect to obtain additional data from other parts

of the globe, which will make the investigation of this somewhat remarkable phenomenon more complete, yet those we now have are sufficient to justify an attempt being made to bring the more important facts before the Royal Society without further delay.

The following table shows the stations from which the records have been received of which use has been made in this discussion, with certain particulars of their geographical position, and of their distances measured on great circles, from Krakatoa, the place of eruption :—

Table I.

Place.	Longitude.	Latitude.	Distance from Krakatoa, measured on a great circle.	
			From west to east.	From east to west.
	° ′	° ′	° ′	° ′
Toronto	W. 79 15	N. 43 40	217 45	142 15
Valencia	„ 10 18	„ 51 55	249 31	110 29
Coimbra	„ 8 24	„ 40 13	247 58	112 2
Armagh	„ 6 39	„ 54 21	252 17	107 43
Falmouth	„ 5 4	„ 50 9	252 15	107 45
Glasgow	„ 4 18	„ 55 53	253 57	106 3
Stonyhurst	„ 2 28	„ 53 51	254 34	105 26
Aberdeen	„ 2 6	„ 57 10	255 25	104 35
Kew	„ 0 19	„ 51 28	255 27	104 33
Greenwich	„ 0 0	„ 51 29	255 39	104 21
Paris	E. 2 20	„ 48 50	256 49	103 11
Brussels	„ 4 20	„ 50 51	258 17	101 43
St. Petersburgh	„ 30 20	„ 59 56	272 3	87 57
Krakatoa	„ 105 22	S. 6 9		

As the earlier disturbances, on the 27th and 28th August, extend over several hours, it became necessary to fix on certain sufficiently well-defined points in the curves representing the barometric pressure, from which to measure the epochs of the passage of successive disturbances. The *first* and *second* of the series are, in almost all the curves, well defined and generally similar in form, commencing with a distinct rise, which is again followed by a distinct fall, the fall being shorter than the rise. These features are followed by a less definite rise succeeded by a shallow fall, after which there is again a rise, which gradually passes into the more regular trace.

The *third* and *fourth* of the disturbances can be traced in all the curves, but they no longer exhibit the same characters, and are usually nothing more than a sudden sharply defined rise, though in front of some of these there is a more or less distinct trace of a hollow.

The *fifth* and *sixth* of the series become less distinct and are lost at several stations, being usually rises; while a *seventh* faint dis-

turbance, as a shallow hollow, can be traced in a few of the curves, after which nothing can be distinguished.

By a comparison of the time intervals between the first and third, the third and fifth, and the fifth and seventh disturbances, and assuming (which the facts seem to justify) that the velocity of the wave has remained unchanged in its passage from east to west, it would appear that the first well-defined rise in the first of the series corresponds to the rises which are prominent in those succeeding it. And the same conclusion has been drawn from an examination of the second and fourth compared with the fourth and sixth of the series.

Adopting these conclusions, the times of the successive passages of the initial rise have been measured from the curves, suitable allowance having been made where the rise was difficult to trace, or as sometimes happened, a hollow appeared corresponding in position with the hollows in the earlier form of the disturbances. There is, of course, some doubt attaching to these measurements, but their general consistency seems to indicate that they may be accepted as fairly representing the facts under discussion.

The following table gives the results of these estimates of the times at which the successive waves passed the several stations, reckoned from midnight of the 26th August, in Greenwich mean time.

Table II.

Place.	Times of passage of wave.						
	I.	II.	III.	IV.	V.	VI.	VII.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Toronto	16 55	25 10	55 10	61 30			
Valencia	13 55	26 30	50 50	62 5	87 55	96 10	124 45
Coimbra	13 50	26 55	50 30	62 40			
Armagh	13 30	26 45	50 40	62 15	87 45	96 20	124 30
Falmouth	13 25	27 0	50 25	62 15	..	97 45	124 30
Glasgow	13 30	27 0	50 35	62 20	87 35	97 30	
Stonyhurst.......	13 20	26 50	50 25	62 25	87 40	97 30	124 5
Aberdeen	13 20	27 5	50 30	62 30	87 20	98 30	
Kew..............	13 15	27 15	50 15	62 30	..	98 0	124 5
Greenwich	13 15	27 15					
Paris	13 15	27 30	50 0	62 50			
Brussels	12 35	27 45	50 0	62 55	86 45	98 40	
St. Petersburgh ...	11 15	28 40	48 30	63 50	84 40		

From these figures are deduced the intervals between the successive passages of the waves from east to west, and from west to east,

respectively, or of the times of travelling round the earth, which are shown in Table III, for all the stations except Toronto.

Table III.

Place.	Intervals occupied in travelling round the earth						
	From east to west.				From west to east.		
	I to III.	III to V.	V to VII.	Mean.	II to IV.	IV to VI.	Mean.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Valencia	36 55	37 5	36 50	36 57	35 35	34 5	34 50
Coimbra	36 40	..	..	36 40	35 45	..	35 45
Armagh.......	37 10	37 5	36 45	37 0	35 30	34 5	34 48
Falmouth	37 0	37 3*	37 3*	37 2	35 15	35 30	35 22
Glasgow	37 5	37 0	..	37 3	35 20	35 10	35 15
Stonyhurst	37 5	37 15	36 25	36 55	35 35	35 5	35 20
Aberdeen	37 10	36 50	..	37 0	35 25	36 0	35 43
Kew	37 0	36 55*	36 55*	36 57	35 15	35 30	35 23
Greenwich	36 45						
Paris	36 45	..	..	36 45	35 20	..	35 20
Brussels	37 25	36 45	..	37 5	35 10	35 45	35 28
St. Petersburgh	37 15	36 10	..	36 43	35 10	..	35 10
Mean excluding Toronto ..	37 4	36 54	36 48	36 57	35 24	35 9	35 17

* At these stations the fifth transit cannot be traced.

From the results thus obtained it would follow that the wave travelled round the earth from east to west in 36 h. 57 m., being at the rate of ·1026 hour for one degree of a great circle of the earth, and from west to east in 35 h. 17 m., being at the rate of ·098 hour for one degree. From the velocities thus determined the probable time of the origin of the wave has been calculated from the known distance of each place from Krakatoa, the time occupied in the passage of the wave from Krakatoa to the place of observation, and the observed time of the passage of the waves.

The mean value thus obtained from the waves moving from east to west for the time of the origin of the disturbance at Krakatoa is 2·52 h. Greenwich mean time, or 9·53 h. local time, that is 9 h. 32 m. A.M. of the 27th August.

In like manner the waves travelling from west to east will give the following results (Table V) :—

The mean value of the time of the origin of the disturbance obtained from the waves moving from west to east is therefore 2·20 h. Greenwich mean time, or 9·21 h. local time, that is 9 h. 13 m. local time.

Table IV.

Place.	Distance from Krakatoa.	Time required for wave to reach first transit.	Greenwich mean time.		Time to reach third transit.	Greenwich mean time.		Time to reach fifth transit.	Greenwich mean time.		Time to reach seventh transit.	Greenwich mean time.	
			Observed first wave.	Time of origin deduced.		Observed third wave.	Time of origin deduced.		Observed fifth wave.	Time of origin deduced.		Observed seventh wave.	Time of origin deduced.
	°	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	H rs.
Valencia	110·48	11·34	13·92	2·58	48·29	50·83	2·54	85·24	87·92	2 68	122·19	124·75	2·56
Coimbra	112·03	11·49	13·83	2·34	48·44	50·50	2·06	85·39					
Armagh..........	107·72	11·05	13·50	2·45	48·00	50·67	2·67	84·95	87·75	2·80	121·90	124·50	2·60
Falmouth	107·75	11·05	13·42	2·37	48·00	50·42	2·42	84·95	..	..	121·90	124·50	2·60
Glasgow	106·05	10·88	13·50	2·62	47·83	50·58	2·75	84·78	87·58	2·80			
Stonyhurst	105·43	10·82	13·33	2·51	47·77	50·42	2·65	84·72	87·67	2·95	121·67	124·08	2·41
Aberdeen	104·58	10·73	13·33	2·60	47·68	50·50	2·82	84·63	87·33	2·70			
Kew....	104·55	10·73	13·25	2·52	47·68	50·25	2·57	84·63	..	..	121·58	124·08	2·50
Greenwich	104·35	10·71	13·25	2·54	47·66	..	..	84·61					
Paris	103·18	10·59	13·25	2·66	47·54	50·00	2·46	84·49					
Brussels	101·72	10 44	12·58	2·14	47·39	50·00	2·61	84·34	86·75	2·41			
St. Petersburgh ...	87·95	9·02	11·25	2·23	45 97	48·50	2·53	82·92	84·67	1·75			
Mean.......	..	..	..	2·46	..	..	2·55	..	..	2·58	..	..	2·53

Table V.

Place.	Distance from Krakatoa.	Time for wave to reach second transit.	Greenwich mean time. Observed second wave.	Time of origin deduced.	Time for wave to reach fourth transit.	Greenwich mean time. Observed fourth wave.	Time of origin deduced.	Time for wave to reach sixth transit.	Greenwich mean time. Observed sixth wave.	Time of origin deduced.
		Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.	Hrs.
Valencia	249°·52	24·45	26·50	2·05	59·75	62·08	2·33	95·03	96·17	1·14
Coimbra	247·97	24·30	26·92	2·62	59·58	62·67	3·09	94·86		
Armagh	252·28	24·72	26·75	2·03	60·00	62·25	2·25	95·28	96·33	1·05
Falmouth	252·25	24·72	27·00	2·28	60·00	62·25	2·25	95·28	97·75	2·47
Glasgow	253·95	24·89	27·00	2·11	60·17	62·33	2·16	95·45	97·50	2·05
Stonyhurst	254·57	24·95	26·83	1·88	60·23	62·42	2·19	95·51	97·50	1·99
Aberdeen	255·42	25·03	27·08	2·05	60·31	62·50	2·19	95·59	98·50	2·91
Kew	255·45	25·03	27·25	2·22	60·31	62·50	2·19	95·59	98·00	2·41
Greenwich	255·65	25·05	27·25	2·20	60·33	..	..	95·61		
Paris	256·82	25·17	27·50	2·33	60·45	62·83	2·38	95·73		
Brussels	258·28	25·31	27·75	2·44	60·59	62·92	2·33	95·87	98·67	2·80
St. Petersburgh	272·05	26·66	28·67	2·01	61·94	63·83	1·89	97·22		
Means	..	..	..	2·19	..	..	2·30	..	..	2·10

The mean between the two values obtained from the waves travelling against the earth's motion of revolution and those travelling with it is 2 h. 24 m. Greenwich mean time, or 9 h. 24 m. local time, 27th August.

The velocity of the waves in miles will be for those which travel from east to west 674 miles per hour, and for those passing from west to east 706 miles per hour. The velocity of sound is for a temperature of 50° F. 757 miles an hour, and for 80° F. 781 miles an hour. With a temperature as low as zero F. the velocity will only be reduced to 723 miles an hour, which is still considerably in excess of the greater of the observed velocities. The excess of the velocity of the waves which travelled in the same direction as the earth's motion of revolution, that is, from west to east, over that of those which passed in the opposite direction, is about 32 miles an hour, which might be accounted for by the circumstance that the winds along the paths of the waves would, on the whole, be from the west, which would cause an increase in the velocity of the one set, and a diminution in that of the other, so that the observed difference of 32 miles would correspond to an average westerly wind of 16 miles an hour, which is not improbable.

It should be observed that the path of the wave which passed Toronto approached very near to the North and South Poles, and that the velocity in both directions appears to be somewhat less than in the waves which passed over central Europe. The wave which passed northwards over Asia travelled at the rate of about 660 miles an hour, or about 15 miles an hour slower than the wave which passed over Great Britain from east to west. This reduction of velocity seems to be within the limits of what might be due to the low temperature of the regions.

The wave travelling from east to west having been perceptible on the barometer traces at several of the stations until about 122 hours after its origin, and its velocity having been 674 miles an hour, it had travelled before its extinction more than 82,200 miles, and had passed 3¼ times round the entire circuit of the earth.

It is further worthy of notice that during the 30th and 31st of August and 1st September, a very severe cyclonic storm was crossing the North Atlantic, and that the wave coming from the westward early on the 31st, No. VI of the series, must have passed on in front of the cyclone, and that its next transit would have carried it into the very centre of the cyclone near the British Isles on the afternoon of the 1st September. This perhaps accounts for no trace of it being found, though the wave coming from the eastward on the morning of that day, just before the cyclone had arrived, No. VII, was discernible.

There is no definite statement, so far as I am informed at present, of the true time of any particularly severe shock or explosion at

Krakatoa excepting that which is contained in the letter of Mr. Watson (published in "Nature," 6th December, 1883), whose ship was within a few miles of the volcano on the morning of the 27th August. He refers to an unusually severe explosion as having occurred at 11 h. 15 m. A.M. local time, which is nearly 45 minutes later than the time, 9 h. 32 m., arrived at in the foregoing discussion. The point of the disturbance (as indicated by the barograms) which has been taken as the front of the wave is the highest point of the first abrupt rise of the trace, and is perhaps on an average not far from one hour after the first signs of disturbance, the increase of pressure having been very rapid during the interval, but broken into two or three steps or oscillations. During the following half-hour there is usually a large decrease of pressure, succeeded by another abrupt rise lasting about half an hour. Then follow a fall of about an hour, then a rise of an hour and a half, and then a fall of an hour and a quarter. The whole length of the disturbance on the time scale is between five and six hours, corresponding to an actual distance of between 3500 miles and 4000 miles. The length of the first main wave of the disturbance is about one hour on the time scale or about 700 miles in length over the earth's surface.

In the present position of our knowledge of the facts, it can only be surmised that the shock of 11 h. 15 m. A.M. of the 27th August observed by Mr. Watson corresponds to the second main feature of the disturbance. That the wave which forms the first feature would have originated at 11 h. 15 m. A.M. is apparently inconsistent with the observed velocities, which it has been shown are remarkably consistent, and indicate without much doubt an origin at 9 h. 32 m. A.M.

The barometric disturbance at Mauritius noted by Dr. Meldrum is said to have begun soon after 11 A.M. local time. The distance from the volcano to Mauritius being about 3450 miles, the wave at the rate of 674 miles per hour would have reached the island in 5 h. 7 m. Taking the great shock at 2 h. 32 m. Greenwich mean time, as before reckoned, the wave would reach Mauritius at 7 h. 39 m. Greenwich mean time, or adding the allowance for difference of longitude, 3 h. 50 m., the local time would be 11 h. 29 m., which agrees satisfactorily with the facts as recorded.

In conclusion, it may be noticed that the sea-waves produced by this volcanic disturbance, assuming the time of its occurrence to have been 2 h. 32 m. Greenwich mean time of the 27th August, were propagated with an approximate velocity of 480 miles an hour to Mauritius, of 430 miles an hour to Port Elizabeth near the Cape of Good Hope, and 420 miles to Galle, and a somewhat slower rate to Aden. The details of the occurrence of these waves on the coasts of India will shortly be laid before the Society by Major Baird, who has informed

me that the velocity of the wave between Galle and Aden was 378 miles an hour, and the lengths of the great waves from 287 to 630 miles.

Postscript, December 15.—Since the above was read before the Royal Society, a copy of the barometric trace from New York has been received, which shows disturbances very similar to those recorded at Toronto, and at times which are quite in accordance with the conclusions stated in the paper.

III. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery." By WARREN DE LA RUE, M.A., D.C.L., Ph.D., F.R.S., and HUGO W. MÜLLER, Ph.D., F.R.S. Received December 5, 1883.

SECOND POSTSCRIPT TO PART IV. "PHIL. TRANS.," PART II, VOL. 174.

Striking Distance.

In a postscript to Part IV of our researches,* we stated that, with 14,400 cells, partly of the rod form, partly of the chloride-in-powder form, the length of the spark between paraboloidal points was 0·7 inch (17·8 millims.), and between a point and disk 0·62 inch (15·7 millims.), and that it does not appear, therefore, that the law of the spark being as the square of the number of cells holds good beyond a certain number.

These results were obtained at the Royal Institution; since the removal of the battery to our laboratory we had not, at the date of the postscript to Part IV of our researches, charged up the whole of it. Recently, however, we have put the battery in thorough order, by scraping the zinc rods† of the cells already charged up and added newly made up cells to bring up the total to 15,000 cells, all of the rod form.

Having the whole 15,000 cells in perfect order, we thought that it would be desirable to make fresh determinations of the striking distance, increasing the potential a thousand cells at a time, between two very slightly convex disks (planes), a point and disk, and two paraboloidal points. These points are one-eighth of an inch (3·175 millims.) in diameter, and three-eighths of an inch (9·525 millims.) long. In the case of a point and disk, the point was like one of those used for

* "Phil. Trans.," Part II, vol. 174, p. 725, separate copy p. 249.

† We are at present making experiments in order to prevent the deposit of oxychloride of zinc on the zinc rods by covering the charging fluid with a layer of paraffin oil.

two points, and the disk was $1\frac{5}{16}$ inch [3·334 centims.] in diameter. The two planes used were $1\frac{5}{16}$ inch [3·334 centims.] in diameter.

As the points, particularly the negative, are deformed at each discharge, the precaution was taken to touch up the point after each discharge in the shaping-tool, screwed to the mandril of the lathe, mentioned in Part I of our researches,* and thus to restore it to a true paraboloidal form.

The following results were obtained between:—

Table I.

Two Disks.

Cells.	Striking distance. Inch.	Striking distance. Centim.
12,000	0·148	0·3759
13,000	0·160	0·4191
14,000	0·181	0·4597
15,000	0·198	0·5029

Table II.

A Point and a Disk.

Cells.	Striking distance. Inch.	Striking distance. Centim.
1,000	0·0055	[illegible]
2,000	0·0240	[illegible]
3,000	0·0600	[illegible]
4,000	0·0950	[illegible]
5,000	0·1700	[illegible]
6,000	[illegible]	[illegible]
7,000	[illegible]	[illegible]
8,000	[illegible]	[illegible]
9,000	[illegible]	[illegible]
10,000	[illegible]	[illegible]
11,000	[illegible]	[illegible]
12,000	[illegible]	[illegible]
13,000	[illegible]	[illegible]
14,000	[illegible]	[illegible]
15,000	[illegible]	[illegible]

* "Phil. Trans.," Part I, [illegible]

Table III.

Two Points.

Cells.	Striking distance. Inch.	Centim.
1,000	0·007	0·0178
2,000	0·020	0·0508
3,000	0·052	0·1320
4,000	0·140	0·3555
5,000	0·220	0·5588
6,000	0·273	0·6934
7,000	0·345	0·8762
8,000	0·405	1·0290
9,000	0·480	1·2192
10,000	0·513	1·3030
11,000	0·575	1·4605
12,000	0·614	1·5595
13,000	0·649	1·6490
14,000	0·675	1·7144
15,000	0·740	1·8800

These several results, the different sets being distinguished by plain crosses or crosses with a dot, are laid down on the diagram, Fig. 1, to which are also added other results already published from former experiments; these latter have a ring on one of the members of the cross. The crosses for two disks, up to 11,000 inclusive, are taken from the table in Part III of our researches,* those for the point and disk from Table XI in Part I, p. 84, and the table in the same part, p. 116; those for two points, from Table XIII*a*, Curve XV*a*, p. 86, separate copy p. 32, and the table, p. 118, separate copy p. 64, of Part I of our researches.†

Through the absolute observations, curves were drawn as on the diagram to represent the mean of the experiments in Tables I, II, and III, represented on the diagram by plain crosses (without a ring). From these curves were deduced the numbers given in Tables IV, V, and VI, in C.G.S. units.

* "Phil. Trans.," Part I, vol. 171, p. 241, separate copy p. 177.

† "Phil. Trans.," Part I, vol. 169, separate copy p. 64.

Fig. 1.

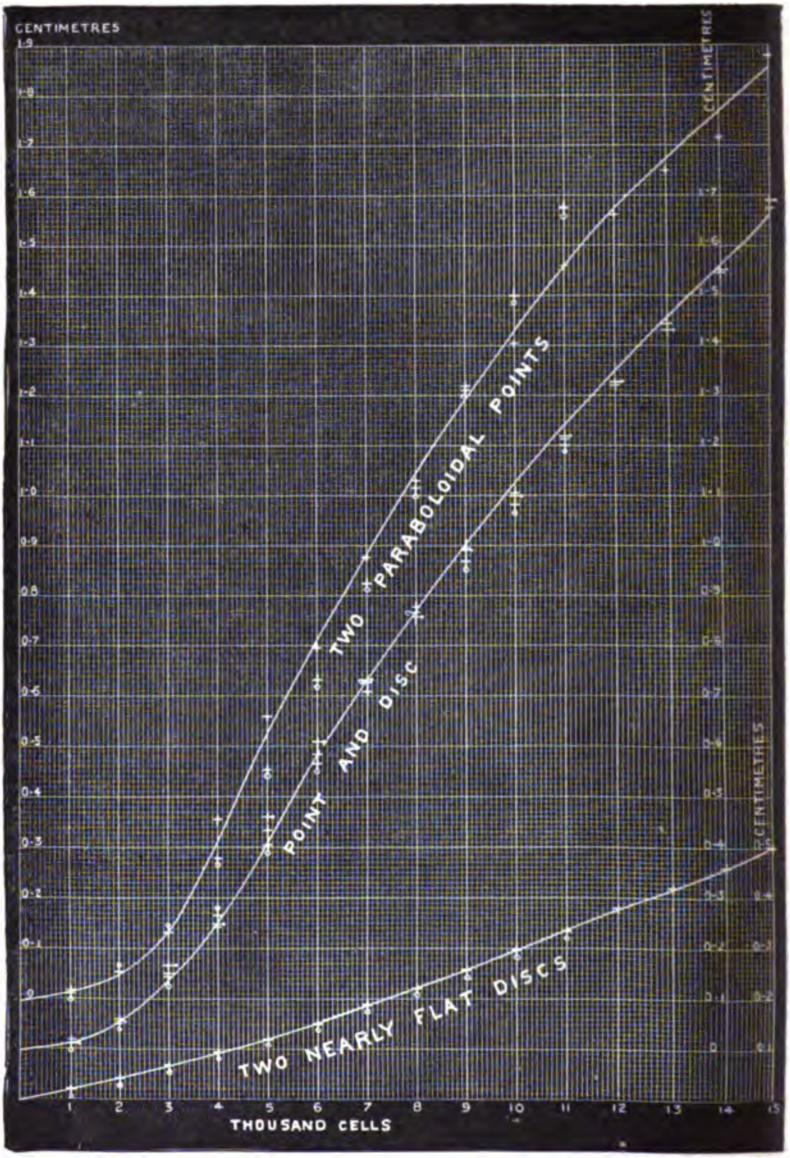

CENTIMETRES
CENTIMETRES
CENTIMETRES
TWO PARABOLOIDAL POINTS
POINT AND DISC
TWO NEARLY FLAT DISCS
THOUSAND CELLS

Table IV.

Two Disks.

E.M.F. in volts.	Striking distance in centimetres.	Difference of potential per centimetre. Volts.	Intensity of force.	
			Electro-magnetic.	Electro-static.
1,000	0·0205	48,770	$4 \cdot 88 \times 10^{12}$	163
2,000	0·0430	46,500	4·65 „	155
3,000	0·0660	45,450	4·55 „	152
4,000	0·0914	43,770	4·38 „	146
5,000	0·1176	42,510	4·25 „	142
6,000	0·1473	40,740	4·07 „	136
7,000	0·1800	38,890	3·89 „	130
8,000	0·2146	37,280	3·73 „	124
9,000	0·2495	36,070	3·61 „	120
10,000	0·2863	34,920	3·49 „	116
11,000	0·3245	33,900	3·39 „	113
12,000	0·3566	33,652	3·37 „	112
13,000	0·4068	31,957	3·20 „	107
14,000	0·4463	31,369	3·14 „	105
15,000	0·4882	30,725	3·07 „	102
15,450	0·5029	30,722	3·07 „	102

Table V.

A Paraboloidal Point and a Disk.

E.M.F. in volts.	Striking distance in centimetres.	Difference of potential per centimetre. Volts.	Intensity of force.	
			Electro-magnetic.	Electro-static.
1,000	0·0123	81,103	$8 \cdot 11 \times 10^{12}$	270
2,000	0·0567	35,274	3·53 „	118
3,000	0·1379	21,755	2·18 „	73
4,000	0·2447	16,347	1·63 „	54
5,000	0·4029	12,410	1·24 „	41
6,000	0·5631	10,655	1·07 „	36
7,000	0·7039	9,945	0·99 „	33
8,000	0·8447	9,471	0·95 „	32
9,000	0·9709	9,270	0·93 „	31
10,000	1·0874	9,196	0·92 „	31
11,000	1·1990	9,174	0·92 „	31
12,000	1·3058	9,190	0·92 „	31
13,000	1·4078	9,234	0·92 „	31
14,000	1·5145	9,244	0·92 „	31
15,000	1·6116	9,307	0·93 „	31
15,450	1·6600	9,307	0·93 „	31

Table VI.

Two Paraboloidal Points.

E.M.F. in volts.	Striking distance in centimetres.	Difference of potential per centimetre. Volts.	Intensity of force.	
			Electro-magnetic.	Electro-static.
1,000	0·0173	57,866	$5·79 \times 10^{12}$	193
2,000	0·0493	40,568	4·06 „	135
3,000	0·1282	23,409	2·34 „	78
4,000	0·3078	12,996	1·30 „	43
5,000	0·5107	9,790	0·98 „	33
6,000	0·6845	8,766	0·88 „	29
7,000	0·8496	8,239	0·82 „	27
8,000	1·0117	7,908	0·79 „	26
9,000	1·1602	7,757	0·78 „	26
10,000	1·2913	7,744	0·77 „	26
11,000	1·3130	7,785	0·78 „	26
12,000	1·5243	7,873	0·79 „	26
13,000	1·6271	7,990	0·80 „	27
14,000	1·7146	8,165	0·82 „	27
15,000	1·7961	8,351	0·84 „	28
15,450	1·8500	8,351	0·84 „	28

An inspection of the diagram, drawn on a reduced scale from the curves as originally laid down, shows that the curve for approximate planes (slightly convex, to ensure the centres being the most prominent) is continuously concave, whereas those for both point and disk and two points are concave only for a certain distance, and then turn off and become convex. Moreover, that the intensity of force per centimetre decreases continuously up to 15,450 volts in the case of planes; but that, in the case of a point and disk, and also in that of two points, the decrease ceases after a certain potential has been reached, and that then it increases so as to become nearly a constant quantity. Between a point and a disk the potential per centimetre at 9,000 volts and beyond is very nearly 9,200; consequently, if the law holds good, to produce a spark 1 décimetre (3·94 inches) long, 92,000 volts, one 1 metre (39·37 inches) long, 920,000 volts,* and a flash of lightning 1 kilometre (0·621 mile) in length, a potential of

* To produce a spark between a point and a disk used for example as the dischargers of an induction coil—

In length.	It would require in E.M.F. volts.
1 inch	23,367
1 foot	280,400
1 yard	841,230

920,000,000 volts would be required, but this potential would be lessened by the diminution of the atmospheric pressure at the height of a kilometre, namely 607·4 millims. (799,210 M), or a mean pressure of 713·8 millims. (939,211 M) between 1 kilometre and the earth. Taking the mean pressure 939,211 M, it would require 864,000,000 volts to produce a discharge between a cloud (regarded as a point) 1 kilometre high and the earth.

It is extremely difficult to conjecture how a cloud can become charged to such an enormous potential, unless the charged molecules balance each other (as those of a stratum in a vacuum tube may be conceived to do) until a disturbing cause breaks up the arrangement; and then the whole of them are discharged in one direction with their aggregate potential.

We may add that less than 15,000 cells would not have sufficed to make out the fact that the intensity of force to produce a discharge between a point and disk or two points becomes a constant after 9,000 to 11,000 cells has been reached.

The following table gives the ratios of the striking distances between a point and a disk and two points respectively, taking those between two disks as unity. And also the relation between the striking distances between a point and a disk and between two points, taking those between a point and a disk as unity.

Cells.	Ratio between point and disk to that between two disks.	Ratio between two points and that between two disks.	Ratio between two points and that between a point and disk.
With 1,000	0·60	0·84	1·40
„ 2,000	1·32	1·15	0·87
„ 3,000	2·09	1·94	0·93
„ 4,000	2·68	3·37	1·26
„ 5,000	3·42	4·34	1·27
„ 6,000	3·82	4·65	1·22
„ 7,000	3·91	4·72	1·21
„ 8,000	3·94	4·71	1·20
„ 9,000	3·89	4·65	1·20
„ 10,000	3·80	4·51	1·19
„ 11,000	3·69	4·35	1·18
„ 12,000	3·58	4·18	1·17
„ 13,000	3·46	4·00	1·16
„ 14,000	3·39	3·84	1·13
„ 15,000	3·30	3·68	1·12
			Mean 1·16

The striking distances from which the above ratios are calculated are those obtained from the smoothed curves.

IV. "On the Figure of Equilibrium of a Planet of Heterogeneous Density." By G. H. Darwin, F.R.S., Plumian Professor of Astronomy and Fellow of Trinity College, Cambridge. Received December 3, 1883.

The problem of the figure of the earth has, as far as I know, only received one solution, namely, that of Laplace.* His solution involves an hypothesis as to the law of compressibility of the matter forming the planet, and a solution involving another law of compressibility seems of some interest, even although the results are not perhaps so conformable to the observed facts with regard to the earth as those of Laplace.†

The solution offered below was arrived at by an inverse method, namely, by the assumption of a form for the law of the internal density of the planet, and the subsequent determination of the law of compressibility. One case of the solution gives us constant compressibility, and another gives the case where the modulus of compressibility varies as the density, as with gas.

It would be easy to fabricate any number of distributions of density, any one of which would lead to a law of compressibility equally probable with that of Laplace; but the solution of Clairaut's equation for the ellipticity of the internal strata of equal density seems in most cases very difficult. Indeed, it is probable that Laplace formulated his law because it made the equation in question integrable, and because it was not improbable from a physical point of view.

The following notation will be adopted:—

For an internal stratum of equal density let—

r be the radius vector of any point,
a the mean radius of the stratum,
e the ellipticity,
w the density,
ϕ colatitude from the axis of rotation,
p the hydrostatic pressure at the point r, ϕ.

For the surface let $\mathfrak{r}$, $\mathfrak{a}$, $\mathfrak{e}$, $\mathfrak{w}$ denote the similar things.

Let M be the mass of the planet, ρ its mean density, ω the angular velocity of rotation, m the ratio $\omega^2/\frac{4}{3}\pi\rho$.

* Since this paper was presented I have seen a reference to a paper by the late M. Edouard Roche, in vol. i of the Memoirs of the Academy of Montpellier (1848), in which the problem is solved, when the rate of increase of the density varies as the square of the radius. See Tisserand, "Comptes Rendus," 23rd April, 1883.

† Laplace's hypothetical law of compressibility arises from a law of internal density for which the problem had previously been worked out, as an example, by Legendre. See Todhunter's "History of the Figure of the Earth," vol. ii, pp. 117 and 337.

Let k be the ratio of the density of the stratum a to the mean density of all the matter situated inside that stratum, and $\mathbf{k}$ the surface value of k.*

Let C, A be the greatest and least principal moments of inertia of the planet about axes through its centre of inertia.

Let œ be the ellipticity which the surface would have if the planet were homogeneous with density ρ, so that $\text{œ}=\frac{5}{4}m$.

The condition that the surface of the planet is a level surface is satisfied by—

$$\mathrm{C}-\mathrm{A}=\tfrac{2}{3}\mathrm{M}\mathbf{a}^2(\epsilon-\tfrac{1}{2}m) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

The condition that the internal surfaces are also surfaces of equilibrium demands that e should satisfy Clairaut's equation—

$$\left(\frac{d^2e}{da^2}-6\frac{e}{a^2}\right)\int_0^a wa^2da+2wa^2\left(\frac{de}{da}+\frac{e}{a}\right)=0 \quad . \quad . \quad . \quad . \quad (2).$$

It may be proved from (2), and the consideration that w must diminish as a increases, that e cannot have a maximum or minimum value.

Also it may be shown that the constants introduced in the integral of this equation must be such that—

$$\frac{\text{œ}}{\epsilon}=\frac{5m}{4\epsilon}=\frac{1}{2\epsilon\mathbf{a}}\frac{d}{da}(ea^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

when a is put equal to $\mathbf{a}$ after differentiation.

The mean density is given by—

$$\mathbf{a}^3\rho=3\int_0^{\mathbf{a}} wa^2da \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

And
$$\mathbf{k}=\frac{\mathbf{w}}{\rho}.$$

Neglecting the ellipticity of the strata, we have the moment of inertia about any diameter of the planet given by—

$$\mathrm{C}=\tfrac{8}{3}\pi\int_0^{\mathbf{a}} wa^4da \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

The ratio of (1) to (5) gives the precessional constant. The pressure and density are connected by the equation—

$$\int_a^{\mathbf{a}}\left(\frac{1}{w}\frac{dp}{da}+4\pi wa\right)da+\frac{4\pi}{a}\int_0^a wa^2da=0 \quad . \quad . \quad . \quad . \quad (6).$$

* k, $\mathbf{k}$, are the reciprocals of f, $\mathbf{f}$, according to the notation adopted in Thomson and Tait's "Nat. Phil." (edit. of 1883), § 824.

Now if ϖ be a function such that $wdp=d\varpi$, the differentiation of (6) leads to—

$$\frac{d\varpi}{da}+\frac{4\pi}{a^2}\int_0^a wa^2da=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

and a second differentiation to—

$$\frac{d^2}{da^2}(\varpi a)+4\pi wa=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

It is well known that Laplace assumed that the modulus of compressibility of rock varies as the square of the density. Since this modulus is wdp/dw, Laplace's hypothesis makes ϖ proportional to w, and the equation (8) is at once soluble.

After the determination of w as a function of a, the solution of all the other equations follows.

In this paper I propose to find a new solution, and to compare the results with those of Laplace.

In order to simplify the analysis let the unit of length be equal to the mean radius a of the planet, and the unit of time be such that the surface density w of the plant is also unity.

Now let us assume that the law of internal density is—

$$w=a^{-n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

Then the mean density of all the matter lying inside of the stratum a is $a^{-n}/(1-\frac{1}{3}n)$. Hence, by definition we have—

$$k=1-\tfrac{1}{3}n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

Thus we see that k is a constant for all strata, and therefore also for the surface. In Laplace's theory k is variable. With our assumed law of density and the special units, ρ the mean density is equal to the reciprocal of k.

It is clear that n must be positive, otherwise heavier strata lie above lighter, and it must be less than 3 in order to avoid infinite mass at the centre of the planet.

Now let us find the law connecting pressure and density, and the modulus of compressibility.

Equation (7) becomes

$$\frac{d\varpi}{da}=-\tfrac{4}{3}\pi\frac{a^{1-n}}{1-\frac{1}{3}n},$$

and by definition of ϖ and the assumption (9),

$$\frac{dp}{da}=-\tfrac{4}{3}\pi\frac{a^{1-2n}}{1-\frac{1}{3}n}.$$

Integrating this, with the condition that the pressure vanishes at the surface, we have,

$$p=\frac{2\pi}{3(1-n)(1-\frac{1}{3}n)}[1-a^{2(1-n)}],$$

$$=\frac{2\pi}{3(1-n)(1-\frac{1}{3}n)}[1-w^{-2\frac{1-n}{n}}],$$

whence the modulus of compressibility is

$$w\frac{dp}{dw}=\tfrac{4}{3}\pi\frac{1}{n(1-\frac{1}{3}n)}w^{-2\frac{1-n}{n}}.$$

The case of $n=1$ is interesting; it gives a constant modulus of compressibility equal to 2π, and the law of pressure $p=2\pi\log w$.

If n be less than unity the compressibility, or reciprocal of the modulus, increases with the density, which is of course physically improbable. If n be greater than unity and less than 3, the compressibility becomes less the greater the density. The assumed law probably does not give such good results as those of Laplace, because the decrease of compressibility with increasing density is not sufficiently rapid.

The range of $n=3$ to $n=1$ gives the results which possess most physical interest.

In comparing results with those of Laplace there will be occasion to express the modulus as a length; that is to say, we are to find the length of a column of unit section whose weight (referred to the surface gravity of the planet) is equal to the force specified in the modulus.

Now if g be gravity

$$g=\tfrac{4}{3}\pi\rho=\tfrac{4}{3}\pi\frac{1}{1-\frac{1}{3}n}.$$

Hence the modulus is $\frac{g}{n}w^{2\frac{n-1}{n}}=g\mathfrak{a}\mathfrak{w}\times\frac{1}{n}\left(\frac{w}{\mathfrak{w}}\right)^{2\frac{n-1}{n}}$, the units $\mathfrak{a}$, $\mathfrak{w}$ being reintroduced to give the expression the proper dimensions. Now $g\mathfrak{a}\mathfrak{w}$ is a pressure, and therefore the length of the modulus is $\frac{\mathfrak{a}}{n}\left(\frac{w}{\mathfrak{w}}\right)^{2\frac{n-1}{n}}$. Thus the surface matter has a length modulus equal to $\mathfrak{a}/n$.

Now let us find the ellipticity of the internal strata.

Substituting for w from (9) in (2), we have,

$$a\frac{d^2e}{da^2}+2(3-n)\frac{de}{da}-2n\frac{e}{a}=0.$$

If the solution be assumed of the form $e=ca^{\beta}$, β must satisfy

$$\beta(\beta-1)+2(3-n)\beta-2n=0,$$

whence $$\beta=-(\tfrac{5}{2}-n)\pm\sqrt{\{(\tfrac{5}{2})^2-n(3-n)\}}.$$

Now $n(3-n)$ is a maximum when it is equal to $(\frac{3}{2})^2$, and therefore the square root can never become imaginary.

From the sign of the last term in the equation for β, it is clear that one of the values of β is negative. Hence to avoid infinite ellipticity at the centre, the c corresponding to the negative root must be zero. Hence the solution of Clairaut's equation (2) is

$$e=\epsilon a^{n-\frac{5}{2}+\sqrt{[(\frac{5}{2})^2-n(3-n)]}}.$$

The surface value of $\dfrac{1}{\epsilon}\dfrac{d}{da}(ea^2)$ is clearly $n-\frac{1}{2}+\sqrt{\{(\frac{5}{2})^2-n(3-n)\}}$.

Thus from (3) we have

$$\frac{x}{\epsilon}=\tfrac{1}{2}(n-\tfrac{1}{2})+\tfrac{1}{2}\sqrt{\{(\tfrac{5}{2})^2-n(3-n)\}}.$$

Then substituting for w from (9) in (5),

$$C=\frac{8\pi}{3(5-n)}.$$

And since $M=\frac{4}{3}\pi\rho=\frac{4}{3}\pi\dfrac{1}{1-\frac{1}{3}n}$, we have for the precessional constant

$$\frac{C-A}{C}=\frac{5-n}{3-n}(\epsilon-\tfrac{1}{2}m).$$

Now let us collect these results, and express them in terms of k instead of n. The solution is

$$w=a^{-3(1-k)}.$$

And the mass inside of any radius a is $\dfrac{4\pi}{3k}a^{3k}$.

$$p=\tfrac{2}{3}\pi\frac{w^{\frac{4-6k}{3-3k}}-1}{k(2-3k)}.$$

$$w\frac{dp}{dw}=\tfrac{4}{3}\pi\frac{w^{\frac{4-6k}{3-3k}}}{3k(1-k)}.$$

And when $k=\frac{2}{3}$, $p=2\pi\log w$, $w\dfrac{dp}{dw}=2\pi$, $w=a^{-1}$.

The length of the modulus at the surface is $1/3(1-k)$ of the planet's radius.

$$e=\epsilon a^{\frac{1}{2}-3k+3\sqrt{[(\frac{5}{6})^2-k(1-k)]}}.$$

$$\frac{x}{\epsilon}=\tfrac{5}{4}-\tfrac{3}{2}k+\tfrac{3}{2}\sqrt{\{(\tfrac{5}{6})^2-k(1-k\}}.$$

$$C=\frac{3k}{2+3k}\cdot\tfrac{2}{3}Ma^2.$$

$$\frac{C-A}{C}=\frac{2+3k}{3k}(\epsilon-\tfrac{1}{2}m).$$

Any value from unity to an infinitely small value may be assigned to k, that is to say, we may have any arrangement of density from homogeneity to infinitely small surface density, but if k be greater than $\frac{2}{3}$ the compressibility increases with the density, which is physically improbable.

The infinite density and infinite pressure, which occur in this solution actually *at* the centre, may be avoided by imagining the centre occupied by a rigid spherical homogeneous nucleus, of very small radius δa, and of density $1/k\delta a^{3(1-k)}$.

We have to compare this solution with Laplace's.

For this case k is not constant, and its surface value is $\mathbf{k}$.

Let $\vartheta=\frac{a}{\kappa}$, where κ is a constant, being the arbitrary constant introduced in this solution; and let θ be the surface value of ϑ.*

The solution is—

$$w=\frac{\theta}{\vartheta}\frac{\sin\vartheta}{\sin\theta}.$$

And the mass inside of any radius a is $\frac{4\pi}{3k}a^{3k}$.

$$p=2\pi\kappa^2(w^2-1).$$

$$w\frac{dp}{dw}=4\pi\kappa^2w^2.$$

$$k=\frac{\vartheta^2}{3(1-\vartheta\cot\vartheta)}.$$

The length of the modulus at the surface is $1/(1-\theta\cot\theta)$ or $3\mathbf{k}\theta^{-2}$ of the planet's radius.

$$e=\epsilon\frac{\theta^2}{\vartheta^2}\frac{1-k}{1-\mathbf{k}}.$$

$$\frac{\varpi}{\epsilon}=\frac{\theta^2}{6(1-\mathbf{k})}-\frac{3}{2}\mathbf{k}.$$

$$C=[1-6(1-\mathbf{k})\theta^{-2}]\tfrac{2}{3}Ma^2.$$

$$\frac{C-A}{C}=\frac{1}{1-6(1-\mathbf{k})\theta^{-2}}(\epsilon-\tfrac{1}{2}m).$$

The following table gives the numerical values of the solution, together with columns for comparison with the results of Laplace's theory, for various values of the ratio of surface to mean density.

* See Thomson and Tait's "Nat. Phil.," 1883, § 824.

The case of $\mathfrak{k}=0$ gives the planet infinite mass at the centre, and the values are only inserted in order to complete the series.

$\mathfrak{k}$.	Length mod. at surface in terms of $\mathfrak{a}$ as unity.	$e=\mathfrak{e}a^x$ where $x=$	$\frac{\ae}{\mathfrak{e}}$	Laplace $\frac{\ae}{\mathfrak{e}}$	$\frac{C-A}{C(\mathfrak{e}-\frac{1}{2}m)}$.	Laplace $\frac{C-A}{C(\mathfrak{e}-\frac{1}{2}m)}$.	$p\propto(w^y-1)$. where $y=$
1·0	∞	0·000	1·000	*1·00*	1·667	*1·67*	$-\infty$
·9	3·333	0·132	1·066	*1·04*	1·741	*1·71*	−4·667
·8	1·667	0·293	1·147	*1·09*	1·833	*1·77*	−1·333
·7	1·111	0·488	1·244	*1·15*	1·952	*1·83*	−0·067
·6	1+1·2	0·722	1·361	*1·21*	2·111	*1·90*	0·333
·5	1+1·5	1·000	1·500	*1·27*	2·333	*1·98*	0·667
·4	1+1·8	1·322	1·661	*1·34*	2·667	*2·07*	0·889
·3	1+2·1	1·688	1·844	*1·41*	3·222	*2·17*	1·048
·2	1+2·4	2·093	2·047	*1·48*	4·333	*2·28*	1·167
·1	1+2·7	2·532	2·266	*1·56*	7·667	*2·40*	1·259
·0	1+3·0	3·000	2·500	*1·65*	∞	*2·55*	1·333
·667	1·000	0·562	1·281	*1·17*	2·000	*1·85*	$p\propto\log w$
·333	1+2·0	1·562	1·781	*1·39*	3·000	*2·13*	1·000

Note.—The values in the two columns applicable to Laplace's theory were found by graphical interpolation from a series of values given in "Month. Not.," R.A.S., Dec., 1876, or Thomson and Tait, "Nat. Phil." (1883), § 824′.

In Laplace's theory $p\propto(w^2-1)$, and the modulus of compressibility $\propto w^2$. In the present theory the modulus $\propto w^y$.

The value $\mathfrak{k}=·667$ corresponds to constant compressibility, and $\mathfrak{k}=·333$ to gaseous compressibility.

One of the grounds on which Laplace's solution is held to be satisfactory is that if we take the value of $\ae$, as determined by the known angular velocity and mean density of the earth, and the value of $\mathfrak{e}$ as determined by geodesy, and find the value of $\mathfrak{k}$, the ratio of surface to mean density, which corresponds with the ratio $\ae/\mathfrak{e}$, this same value of $\mathfrak{k}$ is found to give a proper value to the coefficient of $\mathfrak{e}-\frac{1}{2}m$, so as to obtain the observed precessional constant. To be more precise, m is found to be 1/289·66, which gives $\ae=1/231·7$, and $\mathfrak{e}$ has been found to be approximately 1/295. These give $\ae/\mathfrak{e}=1·273$, and this corresponds with $\mathfrak{k}=1/2·06=·49$. This value of $\mathfrak{k}$, with the same values of $\mathfrak{e}$ and m, gives the precessional constant as ·0033, and Leverrier and Serret give its value as ·00327.

Now it appears remarkable that almost as good a correspondence is obtainable from my solution. The value $\ae/\mathfrak{e}=1·273$ corresponds with $\mathfrak{k}=·675$, and when $\mathfrak{k}=·675$ the coefficient of $\mathfrak{e}-\frac{1}{2}m$ in the precessional constant is 1·99, which gives the same precessional constant ·0033.

This value of $\mathfrak{k}$ corresponds very nearly with constant modulus of compressibility, and with pressure determined by $p=2\pi\log w$.

It is claimed in favour of the Laplacian hypothesis that it corresponds to a surface density which is nearly a half of the mean density of the earth, and that we know that average rock has a density of about 2·8. Also it is pointed out in Thomson and Tait's "Natural Philosophy" that the length modulus of compressibility of the surface rock is about 1/4·4 of the earth's radius, which is very nearly the observed length modulus of iron.

These conditions are not well satisfied by the present solution, for the surface density is found to be ·675, or 1/1·48 of the mean density of the planet, whence the specific gravity at the surface is 3·7; and the length modulus at the surface is equal to the planet's radius. It is to be admitted that this density is large, and that the substance is also highly incompressible.

Thus in these respects the Laplacian hypothesis has the advantage. It seems to me, however, that too much stress should not be laid on these arguments. We know nothing of the materials of the earth, excepting for a mile or two in thickness from the surface, hence it is not safe to argue confidently as to the degree of compressibility of the interior. There seems reason to believe that there is a deficiency in density under great mountain ranges, and this would agree with the hypothesis that our continents are a mere intumescence of the surface layers.

According to this view we might expect to find a rather sudden change in density within a few miles of the surface. Now in any theory of the earth's density such a sudden change in the thin shell on the surface could not be taken into account, and the numerical value for the surface density should be taken from below the intumescent layer if it exists. Hence it is not unreasonable to say that a solution of the problem, which gives a higher surface density than that of rock, lies near the truth. I do not maintain that my solution is as likely as that of Laplace, but it is not to be condemned at once because it does not satisfy these conditions as to the density and compressibility of rock.

The two cases which are given at the foot of the above table each possess an interest, the first of constant compressibility, because it corresponds with the case of the earth, and the second of modulus of compressibility varying as the density, because this is the gaseous law.

With constant compressibility the internal ellipticity varies as the ·562 power of a, or nearly as the square root of the radius; with gaseous compressibility it varies as the 1·562 power of a, or nearly as the square root of the cube as the radius.

A numerical comparison of the case of constant compressibility with Laplace's solution for $k=\frac{1}{2}$ gives the following results :—

	$a =$	0	$\frac{1}{4}\mathfrak{a}$.	$\frac{1}{2}\mathfrak{a}$.	$\frac{3}{4}\mathfrak{a}$.	$\mathfrak{a}$.
(Laplace)	$\frac{e}{\mathfrak{e}} =$	0	·812	·844	·902	1·000
(Constant compress.)	$\frac{e}{\mathfrak{e}} =$	0	·459	·693	·851	1·000

Thus the Laplacian solution attributes much higher ellipticity to the internal strata. The solution with constant compressibility in fact gives so large a proportion of the mass in the central region, that attraction has a greater influence compared with rotation, than in the solution of Laplace.

[P.S.—If, as is not improbable, the increase of density in the interior of the earth is due rather to the heavier materials falling down to the centre than to great pressure compressing the material until it has a high density, then the determination of a modulus of compressibility would be fallacious, and it would be more logical to leave the expressions for the pressure and the density both as functions of the radius, without proceeding to eliminate the radius and to form an expression for the modulus of compressibility. I owe this suggestion to a conversation with Sir William Thomson.—December 19, 1883.]

December 20, 1883.

THE PRESIDENT in the Chair.

The Right Hon. Lord Justice Sir Edward Fry was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a Magnetic Balance, and Experimental Researches made therewith." By Professor D. E. HUGHES, F.R.S. Received December 10, 1883.

In a paper "On the Molecular Rigidity of Tempered Steel,"* I advanced the theory that the molecules of soft iron were comparatively free as regards motion amongst themselves, whilst in hard iron or steel they were extremely rigid in their relative positions.

I have since widened the field of inquiry by observing the effects of mechanical compression and strains, as well as annealing and tempering, upon the magnetic capacity of sixty varieties of iron and steel, ranging from the softest Swedish iron to the hardest tempered cast steel.

We know already that soft iron will take a higher degree of temporary magnetism than steel, and that tempered steel retains magnetism more than soft iron; consequently, we might believe that by the aid of an instrument which should give correct measurements, we might be able to include all varieties of iron and steel between the two extremes of softness, as in annealed iron, and hardness, as in high-tempered cast steel. This proved, however, not to be the case, if the iron and steel were not all annealed to one absolute standard, and if magnetised to or near saturation.

In a late paper upon the theory of magnetism,† I said:—

"During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating

* "Proceedings Institution of Mechanical Engineers," January, 1883.

† Society of Telegraph Engineers, May 24, 1883.

through a small degree with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus."

In order to observe this in electro-magnets, we must employ an extremely feeble current, such as from one Daniell cell, with an exterior resistance of from 10 to 1,000 ohms, and we then find the following laws hold with every variety of iron and steel:—

1st. That its magnetic capacity is directly as its softness or molecular freedom.

2nd. That its resistance to a feeble external magnetising force is directly as its hardness, or its molecular rigidity.

This has proved to be the case upon sixty varieties of iron and steel furnished me direct from the manufacturers, and it was remarked that each variety of iron or steel has a certain point, beyond which annealing cannot soften, nor temper harden; consequently, if all varieties were equally and perfectly annealed, each variety would have its own magnetic capacity; or, its specific degree of value when perfectly annealed or tempered, by means of which, we could at once determine its place and quality.

If in place of several varieties we take a single specimen, say hard-drawn Swedish iron wire, and note its magnetic capacity, we find that its value rises rapidly with each partial annealing, until an ultimate softness is obtained; being the limit of its molecular freedom. We are thus enabled to study the best methods of annealing, and to find at once the degree of softness in an unknown specimen. A similar effect occurs in observations upon tempering; from the softest to the hardest temper, until we arrive at its ultimate molecular rigidity.

We have thus in each piece of iron or steel a limit of softness and hardness. In soft Swedish iron tempering hardens but 25 per cent., whilst mechanical compression (such as hammering) hardens it 50 per cent. In cast steel tempering hardens it 400 per cent., whilst mechanical compression gives but 50 per cent. Between cast steel and Swedish iron we find a long series of mild steel and hard iron, varying in their proportionate degree between the two extremes mentioned.

In order to carry out these researches, I constructed an instrument which I have called the magnetic balance. It consists of a delicate silk fibre suspended magnetic needle, 5 centims. in length, its pointer resting near an index having a single fine black line or mark for its zero, the movement of the needle on either side of zero being limited to 5 millims. by means of two ivory stops, or projections. When the north end of the needle and its index zero are north, the needle rests at its index zero, but the slightest external influence, such as a piece of iron 1 millim. in diameter 10 centims. distant, deflects the needle

to the right or left according to the polarity of its magnetism, and with a force proportional to its power. If we place on the opposite side of the needle at the same distance a wire possessing similar polarity and force, the two are equal, and the needle returns to zero, and if we know the magnetic value required to produce a balance we know the value of both. In order to balance any wire or piece of iron placed in a position east and west, a magnetic compensator is used, consisting of a powerful bar magnet free to revolve upon a central pivot, placed at a distance of 30 or more centims., so as to be able to obtain delicate observations. This turns upon an index, the degrees of which are marked for equal degrees of magnetic action upon the needle.

A coil of insulated wire, through which a feeble electric current is passing, magnetises the piece of iron under observation, but as the coil itself would act upon the needle, this is balanced by an equal and opposing coil on the opposite side, and we are thus enabled to observe the magnetism due to the iron alone. A reversing key, resistance coils, and a Daniell cell are required. Great care must be taken so that the electromotive force remains a constant, as a small variation in the electromotive force gives large variations in the readings, and many important details of construction are required, in order that it shall give perfect readings for extremely small magnetic force. Still greater care is required that each specimen of iron or steel shall be annealed to its maximum.

Several methods of observation have been employed with the magnetic balance, the usual one being the one described, but interesting results are attained by observing the influence of earth's magnetism alone on the iron or steel, or we may magnetise all specimens to the same value, and note the amount of current required. We may observe the remaining magnetism after the cessation of the current or the influence of a weak current after the passage of a strong magnetising force. These are more applicable to researches upon the cause of magnetism.

By means of this instrument I have tested sixty varieties of iron and steel, mostly in the form of wires, a wire 1 millim. diameter, 10 centims. long, being the standard used. In all comparative experiments we require one standard form, to which all the rest must be similar in form and size; at present, we cannot readily compare a square or flat bar with a piece of wire, but if all pieces have the same form, and all are annealed to the same standard, then any difference observed between them must be due to their comparative softness, from which we can deduce its quality and place on the line from soft iron to cast steel.

Annealing not only produces softness and consequent molecular freedom, but it entirely frees it from all strains previously introduced

by drawing or hammering. Thus a bar of iron drawn or hammered has a peculiar structure, say a fibrous one, which gives a greater mechanical strength in one direction than another. This bar, if thoroughly annealed at high temperatures, becomes homogeneous, and has no longer even traces of its previous strains, provided that there has been no actual mechanical separation into a distinct series of fibres.

Table I.

Influence of annealing upon Swedish iron, Sample G.

		Degrees of softness indicated upon the magnetic balance.
Wire, hard drawn, as furnished by the makers		230°
Annealed at black heat		255°
,,	dull red	329°
,,	bright red	438°
,,	yellow	507°
,,	yellow-white	525°

From the above table, we notice that a regular increase of softness occurs, as the temperature at which it is annealed increases, the maximum being at a point under that of fusion.

Numerous methods of annealing were tried, the highest results being obtained when the iron or steel was heated as rapidly as possible to a high temperature, and cooled in a neutral surrounding or atmosphere. The facts regarding annealing as pointed out by the measurement of the magnetic capacity of iron, have, no doubt, been in great measure perceived by ordinary mechanical methods; the results of my own researches may be thus formulated:—

1. The highest degree of softness in any variety of iron or steel is that obtained by a rapid heating to the highest temperature less than fusion.

2. The time of gradual cooling required varies directly as the amount of carbon in alloy.

Thus in chemical pure iron rapid cooling, as in tempering, would not harden it, whilst steel might require several days, even for pieces only 1 millim. diameter. Slow cooling has no injurious effect upon pure iron when cooled in a neutral surrounding, consequently where time is no object, we may employ slow cooling in every case.

A wire or piece of iron, thoroughly annealed, must not be bent, stretched, hammered, or filed, as the hardening effects of a bend are most remarkable, and the mere cleaning of its surface by sand paper hardens its surface several degrees.

The following table shows the effect of annealing upon a series of

wires, kindly furnished me expressly for these experiments by Messrs. Frederick Smith and Co., of Halifax :—

Table II.

Marked.		Magnetic capacity.	
		Bright hard drawn.	Annealed.
G	Best Swedish charcoal iron, 1st variety..	230	525
F	„ „ 2nd „ ..	236	510
T	„ „ 3rd „ ..	275	503
S	Swedish, Siemens-Martin, iron	165	430
H	Puddled iron, best best	212	340
Y′	Bessemer, soft steel..................	150	291
Y	Bessemer, hard steel..................	115	172
Z	Crucible fine cast steel	50	84

The above series contains representative irons and steel of all classes, all other varieties yet tried stand between cast steel and Swedish iron, generally classed as hard cast steel, hard steel, mild steel, hard puddled iron, soft iron, Swedish charcoal iron.

From the above table it will be seen that every wire rises greatly in value by annealing, and that we could not estimate the true magnetic capacity of any iron or steel unless special attention was given that all should be annealed to their maximum.

The influence of tempering upon the magnetic retentivity or molecular rigidity has been shown in every piece of iron or steel examined, the molecular rigidity of tempered cast steel being proportional to its species of temper as shown in Table III.

Table III.

Tempering.

	Magnetic capacity.
Crucible fine cast steel, tempered.	
Bright yellow heat cooled in cold water	28
Yellow red „ „	32
Bright yellow tempered in cold water let down to straw colour	33
Bright yellow tempered in cold water let down to blue	43
Bright yellow tempered in oil	51
Bright yellow tempered in water let down to white ..	58
Red heat tempered in water	66
Red heat tempered in oil	72
Crucible cast steel annealed	84
Swedish charcoal iron annealed	525

Table IV.

Marked.		Electrical resistance per mile of ·040 diameter.	Tensile strength per square inch.	Magnetic capacity.			Chemical analyses.						
				Bright hard drawn.	Annealed.	Tempered hard.	Carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Copper.	Iron.
		ohms.	tons.										
G	Best Swedish charcoal iron 1	191·52	28	230	525	435	0·09	trace	trace	0·012	0·06	trace	99·69
F	" " 2	198·40	30	236	510	415	0·10	trace	0·022	0·045	0·03	trace	99·70
T	" " 3	199·62	31	275	503	395	0·15	0·018	0·019	0·058	0·234	trace	99·44
S	Swedish Siemens-Martin iron	226·32	34	165	430	390	0·10	trace	0·035	0·034	0·324	trace	99·60
H	Puddled iron, best best............	259·92	30	212	340	328	0·10	0·09	0·03	0·218	0·234	0·015	99·11
Y′	Best homogeneous soft Bessemer steel	266·32	35	150	291	255	0·15	0·018	0·092	0·077	0·72	trace	98·74
Y	" hard "	312·69	50	115	172	60	0·44	0·028	0·126	0·103	1·296	trace	98·20
Z	Fine crucible cast steel	350·08	55	50	84	28	0·62	0·06	0·074	0·051	1·584	trace	97·41

The

Table IV gives the complete results of the mechanical, chemical, and physical tests upon a series of wires furnished by Messrs. Frederick Smith and Co., of Halifax.

The tensile strength and electric conductivity are those furnished me by Messrs. Smith and Co. The chemical analyses by Mr. Henry S. Bell, of Sheffield, the magnetic capacity of the bright hard drawn, annealed, and tempered wires, were determined by myself by the aid of the magnetic balance.

In the above Table IV there is a complete relation between electric conductivity and magnetic capacity, both progressing in a similar ratio and agreeing in a most remarkable manner.

We see here that the electric conductivity and magnetic capacity have a complete relation to each other, but while in every wire measured I have found this true, it is only so when the wire has been completely annealed and free from mechanical strain, and a feeble magnetic force employed, thus the relation exists only in the limited sphere of elastic rotation already mentioned.

I believe the relation here shown between electric conductivity and magnetic capacity to be of theoretical importance and of practical utility, as we at once find not only the electric conductivity of iron and steel from a simple reading of their magnetic capacity, but also the iron most suitable for the cores of electro-magnets.

II. "Report on the Circumpolar Expedition to Fort Rae." By Captain H. P. DAWSON, R.A. Communicated by Professor G. G. STOKES, Sec. R.S. Received December 12, 1883.

On the 14th April, 1882, I was informed that I was appointed to the command of the Circumpolar Expedition. I at once proceeded to London, and was occupied until the day of sailing in practice with the magnetic instruments at the Kew Observatory, and the purchase of stores, &c., for the expedition.

On the 1st May, Sergeant F. W. Cooksley, Royal Horse Artillery, and Gunner C. S. Wedenby, Royal Artillery, and on the 6th May Sergeant Instructor of Gunnery J. English, R.H.A., reported themselves to me, and commenced attendance at Kew for instruction.

Journey to Fort Rae.

We sailed from Liverpool on the 11th May, and arrived at Quebec on the 23rd. Here I spent some days finding that the steamer for the north did not leave Winnipeg till the 10th June, and my party was

very kindly afforded quarters in the citadel by Lieutenant-Colonel Cotton, commanding the Canadian Artillery at that place.

Having obtained a free pass for our baggage on the Grand Trunk Railway, I started at once for Winnipeg, proceeding by the lakes, that being the cheaper route, and the one which on the whole exposed the instruments to the least knocking about.

We reached Winnipeg on the 9th June, and left on the following day by the Saskatchewan steamer. On the 26th June we reached Carlton, where it was necessary to engage carts to take our baggage to Green Lake, a distance of 140 miles.

On the 29th the carts were taken across the river, and on the 30th we started for Green Lake, which we reached on the 9th July, having been delayed by the extreme badness of the road. The heat of the weather rendered a long halt necessary in the middle of the day, and the flies also prevented our animals from feeding properly, incapacitating them for long marches or fast work, and on one occasion forcing us to halt for a whole day, the oxen being so worried by them as to be unable to march.

At Green Lake we entered upon the system of water communication that forms the only roadway in the north, and by way of Portage la Loche and the Clearwater and Athabasca rivers we reached Fort Chipewyan on the 30th July. Here we had to await the Mackenzie River boats, there being no other means of reaching Fort Rae, and it was not until the 17th August that we were able to start on this last stage of our journey. We reached Great Slave Lake on the 22nd, on the evening of which day a gale arose which stove in and sunk our boat, damaging most of our provisions. Fortunately we were able to repair the boat, but it was not until the 25th that the weather allowed us to proceed, and on the 27th we were again detained by a fresh storm, so that it was not until 10 P.M. on the 30th August, that we arrived at Fort Rae.

Fort Rae.

Fort Rae is situated in lat. 62° 38′ 52″ N., and long. 115° 43′ 50″ W., at the south-west extremity of a peninsula that juts out from the north-east shore of a long gulf extending in a north-westerly direction more than 100 miles from the northern shore of the Great Slave Lake. It is almost entirely surrounded by water, as shown in the annexed plan.* The formation is limestone. The land rises to a height of some 200 feet, and is covered in part with moss, in part with pines and scanty brushwood. A few vegetables are grown in the summer in

* [It has not been thought necessary to publish the plans accompanying this Report, as they would seem to find a more fitting place in the detailed account of the observations.—G. G. S., Sec. R.S.]

the garden attached to the Roman Catholic Mission, but for food the inhabitants chiefly depend upon the produce of the nets, and on deer, which are brought in by the Indian hunters attached to the post.

On arrival it was found that the magnetic instruments required a good deal of setting to rights, their boxes being filled with water and the fittings loosened, so that not a single instrument was quite in working order. There was, moreover, no building ready for their reception, so that it was not possible to keep the 31st August—1st September as a term day, but we succeeded in getting the meteorological instruments into position so as to commence observations with them at midnight on the 31st.

We were fortunate in finding a building that admitted of conversion into a magnetic observatory, it only requiring a floor, fire-place, door and windows to be habitable. This work was at once commenced, and on the 3rd September the declinometer, on the 4th the bifilar, and on the 6th the vertical force magnetometer were mounted in their places. This observatory was finished on the 10th September, and another one commenced for astronomical and absolute magnetic observations, the continual wind rendering out-door observations unsatisfactory.

The men of my party were accommodated in the house of one of the sub-officers of the fort, and I had a room in the house of the Hudson's Bay Company's officer in charge.

The instruments, on the whole, suffered but little from the journey. One barometer and one thermometer were broken, and the object glasses of the telescopes of most of the magnetic instruments were nearly opaque, the cement joining the two lenses having from some cause or other melted on the journey. Our provisions were more damaged, 190 lbs. sugar, 30 lbs. of tea, all our rice, and most of our baking powder having been destroyed.

The observations were then carried on without interruption until the 31st August, 1883.

Magnetic Observations.

The balance magnetometer was the only magnetic instrument whose performance was not satisfactory, as not only did it frequently get out of adjustment, but in times of magnetic disturbance it often vibrated through so large an arc that exact reading was impossible. The other instruments were remarkably free from vibration, and there was never any difficulty in reading them, but it was found necessary to extend the scale of the bifilar on the side of decreasing force, owing to the great movements of this instrument.

The greatest magnetic disturbance was on the 17th, 18th, and 19th November, 1882, when all the instruments moved at times beyond the

limits of their scales. On the first of these days the difference between the extreme easterly and westerly positions of the declinometer magnet exceeded 10°.

Aurora.

Aurora was observed on almost every clear night, and was usually attended by more or less magnetic disturbance. It did not appear to me, however, that the two phenomena stood in the relation of cause and effect, but rather that they were both due to a common cause. The most marked instance of connexion between the two phenomena consisted in a rapid decrease in both vertical and horizontal magnetic forces which attended a sudden outburst of aurora in the zenith. This was observed on several occasions. The bifilar almost always showed a reduction of horizontal force during a display of aurora. I also think that the declinometer magnet tended to point towards the brightest part of the aurora, but I have not yet had time to make that careful comparison of the auroral and magnetic observations which will be required to decide this point. It was found impossible to obtain photographs either of the aurora or of its spectrum—the latter invariably presented the characteristic yellowish-green line, and occasionally but rarely several other bright lines were visible for a few moments towards the violet end of the spectrum, and once a bright band was seen in the red.

I was also unsuccessful in my attempts to measure the height of the aurora, chiefly from the want of a well-defined point to measure to, also from the fact that some hours were required to prepare for this observation, whereas the appearance of a suitable aurora could not be predicted, and was, in fact, not of frequent occurrence, and then often only lasting a few seconds. For this observation two stations some miles apart should be connected by telegraph and occupied for many days, or even weeks in succession.

Although I paid attention to the point, I never heard any sound from the aurora, save on the occasion mentioned in a former memorandum, but I made many inquiries on the subject from residents in the country, both English and French, and their statements agree so well, both with one another and with what I myself heard, that I am forced to conclude that the aurora is at times audible, and that on these occasions it appears to be, and probably is, very near the earth.

Meteorological Observations.

With regard to the meteorological observations, the station was somewhat unfavourably placed for observations of wind, on account of the hill to the north-east, but as winds from this quarter were rare the effect on the results will not be great, especially as one of the

anemometers was on an island in the lake, in an entirely open situation.

The anemometers did not work quite satisfactorily, being at times choked by ice; but I hope by the comparison of the two satisfactory results may be attained.

The wind was usually either South-east or North-west; and even when it blew from the former quarter, the motion of the upper clouds often showed the existence of a North-westerly current.

The hair hygrometers were found to be useless out of doors in cold weather, on account of the formation of ice on the hair.

The earth thermometers were read every alternate day: the observations were interrupted by a carcajou, or other animal, which extracted the thermometers from their tube for the sake of the fur in which it had been found necessary to envelope them, and broke them all; other thermometers were, however, substituted, and the observations continued. It was found impossible to obtain the temperature of the soil at a greater depth than four feet on account of the rocky nature of the ground.

A series of observations of terrestrial radiation was made by means of a thermometer placed on the surface of the snow, but the almost continual wind detracts much from the value of these readings.

I was told by the residents of the country that the year was an unusually dry one, and certainly the rainfall is remarkably small; they also said that the winter was particularly mild and free from storms, which, from all accounts, and from the journals kept at the fort, seem to be both frequent and severe; as it was, we only experienced one, in February.

A plan is annexed, showing the position of the meteorological instruments, and their relative heights.

Astronomical Observations.

My first determination of the longitude was made by means of lunar distances, and time was found by the method of equal altitudes, but after the observatory was finished both these points were determined by transits, and the first value of the longitude found to be more than a minute in error. The latitude was determined by transit observations in the prime vertical, and is probably within a few seconds of the truth. The longitude may be ten seconds in error. The time was generally correct to within three or four seconds.

A more solidly constructed transit instrument would have been desirable, as it was found that in the cold weather it required so much force to move the telescope of the transit theodolite on its axis, that there was great risk of disturbing the adjustments of this instrument, composed as it is of so many parts.

Food, &c.

Our supply of provisions proved quite sufficient. I had brought enough flour to admit of my issuing the usual ration of ¾ lb. per diem, and tobacco 1 lb. per month to each man. We also had a supply of Chollet's preserved vegetables, and a reserve stock of bacon, besides tea and sugar. Of the latter we were somewhat short, owing to the loss sustained on the journey up. We usually had fresh meat throughout the winter; in the summer we were occasionally reduced to dried meat. During the journey there and back we chiefly lived on pemmican. The Rev. Père Roure, of the Roman Catholic Mission, most kindly furnished us with fresh vegetables and potatoes throughout the summer.

The conduct of the men under my command was everything that could be desired. They took great interest in the observations, and did their best to carry them out with accuracy and punctuality, and were always contented and cheerful, in spite of the inevitable discomforts of their winter quarters and the occasional hardships of the journey.

Return Journey.

We were running great risk of being overtaken by the winter, and therefore lost no time in our departure.

The last hourly observation was made at midnight on the 31st August, 1883, after which the instruments were dismounted and packed, their cases having been previously arranged in readiness outside the observatory. The remainder of the baggage was already in the boat, so that by 2.30 A.M. on the 1st September we were *en route*, and reached Fort Chipewyan on the 17th September, and Portage la Loche on the 4th October, having experienced some delay in surmounting the rapids of the Clearwater, the hard frosts having frozen all the small tributary streams, thus considerably lowering the water in the river.

The boat awaiting us on the south side of the portage was frozen in, but fortunately the wind changed and the ice broke up before our arrival. Had it been otherwise we must have waited until the rivers were thoroughly frozen, and travelling with dog trains possible. In that case we should have been compelled to abandon our instruments and baggage.

On the 21st we reached Carlton on the Saskatchewan, where we were detained a day, the man engaged to transport our baggage across the prairie having refused to proceed. Another man was engaged, and on the 31st October we reached the railway at Qu'Appelle, arriving at Winnipeg the following day. We were fortunate in crossing the prairie with so little difficulty, as at the same time last year it was covered with three feet of snow.

At Winnipeg I remained a couple of days to adjust accounts with the Hudson's Bay Company, and on the 4th November we started for Quebec, going by rail *viâ* Chicago. We reached Quebec on the 8th, and Liverpool on the 20th November.

In conclusion, I have to acknowledge the assistance received from the officers of the Hudson's Bay Company, who spared no trouble in carrying out my wishes, especially Chief Commissioner Grahame at Winnipeg, Chief Factors MacFarlane and Camsell, in charge of the Athabasca and Mackenzie River Districts respectively, and Mr. King in charge at Fort Rae. To their hearty co-operation the success of the expedition is in great part due.

Results of Expedition.

The following is a list of the observations taken at Fort Rae, the result of our year's work there, which I have now the honour to lay before the Royal Society:—

Magnetic.

Hourly—

Declination from 3rd September, 1882, to 31st August, 1883.
Hor. Force „ 4th „ „ „ „
Vert. Force „ 6th „ „ „ „

Term Day—

In accordance with programme laid down by St. Petersburg Conference, from 15th September, 1882, to 15th August, 1883.

Occasional—

Absolute observations of Hor. Force Dip and Declination.

Meteorological.

Hourly—

Barometer. From 1st Sept., 1882, to 31st Oct., 1883.
Dry and wet bulb thermometers „ „ „
Anemometer „ „ „
Wind, clouds, and weather „ „ „
Aurora (when visible) „ „ „
Hair Hygrometer (when in working order).
Terrestrial Radn. (occasionally in clear weather).

Daily—

Max. and Min. Solar and Terrest. Radn. Thermos.
Rain gauge.

Earth Thermometers every two days.

III. "On the Changes in the Gland-Cells of *Dionæa muscipula* during Secretion." By WALTER GARDINER, B.A., Scholar of Clare College, Cambridge. Communicated by W. T. THISELTON-DYER, C.M.G., F.R.S. Received December 18, 1883.

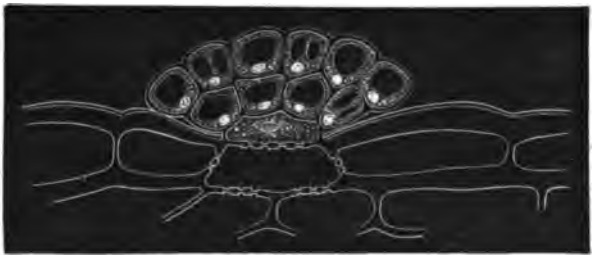
The following observations were made upon leaves of *Dionæa muscipula* which had been fed with the bodies of wood-lice, from which the chitinous coat had been previously removed. The leaves were then placed in absolute alcohol.

It is well known that shortly after the application of animal matter the leaves close, and may remain shut for a period varying usually from ten to twelve days, when they open spontaneously. When, however, the leaf is not vigorous or when the amount of animal matter is somewhat large, the leaf may not open at all, but remain closed until its death.

There appear to be four periods which attend the phenomenon of digestion in such a leaf, viz., the resting, the secretory, and the absorptive periods, and the period of recovery. These periods are fairly well defined in *Dionæa* on account of the slowness with which they proceed.

In the resting state the gland cells (fig. 1) exhibit the following structure:—In each cell, the protoplasm closely surrounds the cell-wall, leaving one large central vacuole, which is filled with the usual pink cell-sap. The protoplasm is extremely granular, especially around the nucleus, which is, in consequence, almost entirely obscured from view. The nucleus is situated at the base of the cell, and is shown by reagents to be large and well-defined.

FIG. 1.



At the end of the secreting period, which appears to be about twenty-four hours after stimulation, the following changes have occurred (fig. 2). Movements of the protoplasm have taken place, in consequence of which the nucleus now occupies the centre of the cell; numerous strands of protoplasm radiate from the nucleus to the parietal protoplasm of the cell, in consequence of which several

vacuoles, instead of one large one, are present. The protoplasm now exhibits but little granularity, and may be described as clear and hyaline. The nucleus is clearly brought into view, and appears to have undergone a very considerable diminution in size.

FIG. 2.

Passing to the phenomena of the ordinary leaf tissue, it may be remarked that definite special cell-contents make their appearance after the absorption of the digested food.

Sections of leaves which were placed in alcohol thirty-six hours after feeding, show that the cells contain a very large number of tufts of crystals, which are present in the cell-vacuole and adhere to the inner surface of the cell-protoplasm. The tufts are formed of fine acicular crystals which crystallise out with great regularity and radiate from a central point. The tufts are of a yellow-green colour. They are insoluble in alcohol, in 1 per cent. acetic acid, and in 1 per cent. hydrochloric acid; and soluble with difficulty in 5 per cent. solution of potash. The formation of these crystals may be artificially produced by wetting the surface of a fresh leaf with the fluid from a leaf which has fed for a period of from thirty-six to forty-eight hours.

After forty-eight hours the cell-contents are of a different nature. The cells now contain numerous bodies which present the appearance of flat sphæro-crystals. They are usually perfectly circular in outline, and are clear and colourless. They are insoluble in alcohol but extremely soluble in water.

In *Drosera* the changes take place much more rapidly, the phenomenon of digestion usually extending over a period of from three to five days. The gland-cells in the resting state were seen to be much more granular before, than after secretion. In consequence of absorption the cells contain a large quantity of a substance which is precipitated in dense granules by alcohol, but is readily soluble in water. The author has not yet worked out *Drosera* in detail.

IV. "On the Continuity of the Protoplasm through the Walls of Vegetable Cells." By WALTER GARDINER, B.A., Scholar of Clare College, Cambridge. Communicated by W. T. THISELTON-DYER, C.M.G., F.R.S. Received December 13, 1883.

Since the communications of November 11th, 1882, and April 16th, 1883, the author has been chiefly employed in testing and improving his methods, and adding to the number of plants in which he has been able to demonstrate the existence of a continuity of the protoplasm between adjacent cells. In certain endosperm cells, *e.g.*, Bentinckia Conda-panna, where the protoplasmic threads traversing the cell walls are particularly well developed, it is possible to see the threads perfectly clearly by merely cutting sections of the endosperm, and mounting them in dilute glycerine. Taking the structure displayed by such a preparation as normal, the author has compared it with the preparations obtained after the action of Chlor. Zinc. Iod. and sulphuric acid. He finds that his method of swelling with Chlor. Zinc. Iod., and staining with Picric-Hoffmann Blue, is in every way perfectly satisfactory, since but little alteration of the structure occurs, and the staining with the Picric-Hoffmann Blue is limited to protoplasm. The sulphuric acid method is in the main unsatisfactory, although it is valuable in the case of thin-walled tissue, where violent swelling must be resorted to; and it is also valuable as affording most conclusive evidence of the existence of a protoplasmic continuity in those cases where the protoplasmic processes of pits cling to the pit-closing membrane. He believes, however, that the results obtained can only be rightly interpreted in the light of the results obtained with Chlor. Zinc. Iod. The possibility of seeing the threads depends upon their degree of tenuity and upon the thickness of the pit-closing membrane, and in extreme cases and in what are by far the more general cases, the only evidence of such perforating threads is afforded by the general staining of the pit-closing membrane. Every transition between clearly defined threads in the substance of the closing membrane and the mere staining of that structure as a whole occurs.

The author has found that in all pitted tissue a pit-closing membrane, which is made evident by staining thin sections with iodine and mounting in Chlor. Zinc. Iod., is uniformly present, and that open pits do not occur. The continuity of the protoplasm is always established by means of fine threads arranged in a sieve-structure, and not by means of comparatively large processes which the occurrence of open pits would necessitate. He cannot therefore agree with observers whose statements necessitate the existence of such open pits.

Since the last communication the author has been able to observe

that a continuity of the protoplasm between adjacent cells occurs in *Dionæa muscipula*, being especially pronounced in the most central layers of parenchymatous cells.

The parenchyma cells of the petioles of certain plants which, as H. von Mohl showed, are often thick walled and conspicuously pitted, afforded favourable material for investigation. In *Aucuba japonica*, and *Prunus lauro-cerasus*, distinct threads could be made out crossing the pit-closing membrane. In *Ilex aquifolium* there was a doubtful striation, and in the rest examined a mere coloration of the pit-membrane.

Examples of continuity have thus been shown to exist in ordinary parenchymatous tissue; and this materially strengthens the belief that the phenomenon of the connexion of cells with one another is one of universal occurrence.

As to the function of the filaments, the author believes that in sieve-tubes and in endosperm-cells they may make possible a transference of solid materials, besides establishing a protoplasmic communication; but in ordinary cells the only significance of the threads is, that by their means the protoplasm of isolated cells becomes connected, and that thus the communication of impulses from one part of the plant to another is insured.

Finally, the presence of these minute perforations of the cell-wall need not lead to any modification of our general ideas as to the mechanics of the cell.

V. "Note on the Constitution of Chlorophyll." By EDWARD SCHUNCK, F.R.S. Received December 6, 1883.

An examination of some products derived from chlorophyll, which has occupied me for some time, has led to the question of the true nature and constitution of chlorophyll, a question on which widely different opinions prevail. Without entering into matters which concern the physiologist only, it may be said that to the chemist chlorophyll is simply an organic colouring-matter, the substance to which the green colour of leaves and other parts of plants is due. Now colouring-matters are of three kinds. To the first class belong such as occur ready formed and in a free state in vegetable and animal organisms, such as the colouring-matters of turmeric and safflower. The second class comprises those that are formed from colourless chromogens by the combined action of alkalis and oxygen, the colouring-matters of log-wood and archil being well-known examples of this class. These colouring-matters change rapidly when exposed to the further action of oxygen in the presence of alkali, but are quite

stable when in contact with acids. The third class consists of glucosides, bodies which do not undergo any considerable change under the influence of alkalis, but are rapidly decomposed when acted on by acids or ferments, yielding, on the one hand, some kind of glucose, and, on the other, substances in which the tinctorial properties of the parent substance are much more pronounced. To this division belong the colouring-matters of madder, quercitron, cochineal, &c. Now chlorophyll in its general properties so much resembles the members of the last class that one cannot help suspecting that to this class it may belong—that it is in fact a glucoside. It shows considerable stability in the presence of alkalis, but acids decompose it rapidly, giving rise to substances which are intensely coloured and show a power of absorbing particular parts of the spectrum much more strongly than chlorophyll itself. Whether, along with the latter bodies, it yields by decomposition with acids some kind of glucose, seemed to me a question worthy of attention.

If it was possible to obtain chlorophyll in a state of purity, it would be very easy to settle this question; unfortunately all attempts hitherto made to separate and purify chlorophyll have ended in its decomposition. I consider it as certain that the so-called crystallised chlorophyll which has been described by several authors is in fact a derivative of chlorophyll formed during the process employed for preparing it. It is, however, very easy to obtain a solution of chlorophyll which shall be quite free from everything soluble in water extracted at the same time from the plant, and therefore free from ready-formed glucose. In order to effect this, I proceed as follows:—Having extracted leaves of any kind with boiling alcohol, I allow the extract to stand for some time, filter off the deposit which usually forms, and then mix it with its own volume of ether and with about two volumes of water, shaking up well. The liquid now separates into two layers, an upper green one, containing all the chlorophyll of the extract, and a lower bright yellow one, which contains tannin, a yellow colouring-matter, a substance giving the glucose reaction with Fehling's solution, and probably other substances besides. The two liquids are separated in the usual way, and the upper one is shaken up with fresh water, which now usually only shows a trace of colour. This process of washing may be repeated, adding each time a little fresh ether, until the lower layer ceases to give the glucose reaction. The upper liquid leaves on spontaneous evaporation a bright green residue, which, though far from being pure chlorophyll, is free from everything soluble in water, and may therefore be employed to determine whether anything soluble in water, such as glucose, is formed by the action of acids on it. If some of the residue be treated with concentrated sulphuric acid in the cold it dissolves, forming a green solution, which, after standing for some time, gives,

on the addition of water, a dark green precipitate. This precipitate consists essentially of two substances, the phyllocyanin and phylloxanthin of Frémy, which are undoubtedly products derived from chlorophyll, showing the absorption bands of what is usually called "acid chlorophyll." The liquid filtered from this precipitate, when mixed with copper sulphate and an excess of caustic alkali, becomes blue, and the mixture, on boiling, deposits cuprous oxide. The experiment may be made in a slightly different manner. The residue left by the green ethereal solution of chlorophyll having been dissolved in alcohol, sulphuric or hydrochloric acid is added to the solution, which is then boiled for some time, evaporated so far as to drive off most of the alcohol, filtered from the products insoluble in water, made alkaline, then mixed with Fehling's solution and boiled, when the usual glucose reaction takes place. In order to make sure that the reaction was not due to ready-formed glucose, I took in every case the precaution of testing a portion of the green chlorophyllic residue with Fehling's solution before acting on the rest with acid. This was easily done by treating with weak alcohol, to which a little alcoholic potash and some Fehling's solution were added, and heating, when the whole dissolved easily, giving a green solution, which, on boiling, in no case deposited the least trace of cuprous oxide, whereas, after adding an excess of hydrochloric acid to the liquid, boiling, filtering off the insoluble products, again making alkaline and boiling, the glucose reaction took place in a marked manner.

This experiment has never in any case failed, and it would follow, if uniformly successful, that the green leaves of all plants contain a glucoside insoluble in water, but soluble in alcohol and ether. That this glucoside is, in fact, chlorophyll seems to me highly probable. Nevertheless, absolute certainty cannot be attained, because the matter experimented on is a mixture, and it is possible that one plant out of many might give a decidedly negative result, which would upset the conclusion drawn from the rest. Assuming, however, that the phenomena will always occur as above described, and that the reaction with Fehling's solution indicates the presence of some kind of glucose, it would follow either that chlorophyll is a glucoside, or that it is always accompanied in the vegetable cell by a glucoside of very similar properties.

I may add that I attempted to isolate the glucose or glucose-like substance formed under the circumstances described, spinach leaves being the material employed, and obtained a pale yellow gum-like substance which showed no tendency to assume a crystalline form.

VI. "On the Physiology of the Carbohydrates in the Animal System." By F. W. PAVY, M.D., F.R.S. Received December 13, 1883.

[Publication deferred.]

The Society adjourned over the Christmas Recess to Thursday, January 10th, 1884.

January 10, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Transfer of Energy in the Electromagnetic Field." By J. H. POYNTING, M.A., late Fellow of Trinity College, Cambridge, Professor of Physics, Mason College, Birmingham. Communicated by Lord RAYLEIGH, M.A., D.C.L., F.R.S. Received December 17, 1883.

(Abstract.)

A space containing electric currents may be regarded as a field where energy is transformed at certain points into the electric and magnetic kinds by means of batteries, dynamos, thermoelectric actions, and so on, while in other parts of the field this energy is again transformed into heat, work done by electromagnetic forces, or any form of energy yielded by currents. Formerly a current was regarded as something travelling along a conductor, attention being chiefly directed to the conductor, and the energy which appeared at any part of the circuit, if considered at all, was supposed to be conveyed thither through the conductor by the current. But the existence of induced currents and of electromagnetic actions at a distance from a primary circuit from which they draw their energy, have led us, under the guidance of Faraday and Maxwell, to look upon the medium surrounding the conductor as playing a very important part in the development of the phenomena. If we believe in the continuity of the motion of energy, that is, if we believe that when it disappears at one point and reappears at another, it must have passed through the intervening space, we are forced to conclude that the surrounding medium contains at least a part of the energy, and that it is capable of transferring it from point to point.

Upon this basis Maxwell has investigated what energy is contained

in the medium, and he has given expressions which assign to each part of the field a quantity of energy depending on the electromotive and magnetic intensities, and on the nature of the matter at that part in regard to its specific inductive capacity and magnetic permeability. These expressions account, as far as we know, for the whole energy. According to Maxwell's theory currents consist essentially in a certain distribution of energy in and around a conductor, accompanied by transformation and consequent movement of energy through the field.

Starting with Maxwell's theory we are naturally led to consider the problem, how does the energy about an electric current pass from point to point; that is, by what paths and according to what law does it travel from the part of the circuit where it is first recognisable as electric and magnetic to the parts where it is changed into heat or other forms.

The aim of this paper is to prove that there is a general law for the transfer of energy according to which it moves at any point perpendicularly to the plane containing the lines of electric and magnetic force, and that the amount crossing unit of area per second of this plane is equal to the product of the two intensities multiplied by the sine of the angle between them divided by 4π, while the direction of flow of energy is that in which a right-handed screw would move if turned round from the positive direction of the electromotive to the positive direction of the magnetic intensity. After the investigation of the general law several applications are given to show how the energy moves in the neighbourhood of various current-bearing circuits.

II. "Some Experiments on Metallic Reflection. IV. On the Amount of Light Reflected by Metallic Surfaces. II." By Sir JOHN CONROY, Bart., M.A. Communicated by Professor STOKES, Sec. R.S. Received December 15, 1883.

In a paper which Professor Stokes did me the honour of communicating to the Royal Society, and which appeared in the "Proceedings," vol. 35, p. 26, I gave an account of some experiments I had made on the amount of light reflected by polished metallic surfaces when ordinary unpolarised light was incident upon them.

The light of a paraffine lamp fell either directly, or after reflection from the metallic surface, on a photometer, and the readings were made by altering the distance at which another similar lamp had to be placed from the photometer in order to produce an equal illumination.

I have repeated the experiments with the steel and speculum metal

mirrors with polarised light. The polish of the tin and silver mirrors being defective, it was not thought worth while to re-examine them.

The general arrangement of the apparatus remained the same; but in order to obtain a more intense light, a magic lantern (the one known as the "Sciopticon" being used) was substituted for the paraffine lamp carried by the goniometer. It was fixed on a wood stage fastened to a stout board about 80 centims. long; a screw, which passed through the board near one end and was fixed in a table, formed an axis about which the board could rotate; the goniometer was fixed to the board with its centre vertically over the axis of rotation.

A black card, with a slit 54 millims. by 4 millims., was placed in the slide-holder of the lantern, and a Nicol in the collimator tube of the goniometer, and the image of the slit focussed on the paper of the photometer.

The metal plates were clamped to the vertical stage, and their adjustment examined by placing a second, or analysing, Nicol in the path of the reflected light and crossing the Nicols, the former being placed with its principal section either in or perpendicular to the plane of incidence, and adjusting the stage screws till the light reflected from the plate was completely extinguished.

The experiments were made in the manner described in the former paper, the light being polarised in, or perpendicularly to, the plane of incidence by the Nicol. It was found that the illumination of the paper varied with the position of the Nicol, being always greatest when the light which fell on the paper was polarised in the plane of incidence.

Table I gives a series of observations made with the steel plate with light polarised in the plane of incidence. The numbers in the first column are the distances, in centimetres, of the sliding lamp from the photometer when the light from the lantern fell directly on the paper; and those in the third when the light was reflected by the mirror. The means of these observations are contained in the second and fourth columns, the angles of incidence in the fifth, and the ratios of the reflected to the incident light—the latter being taken as 100—in the sixth column.

As the intensity of light varies inversely as the square of the distance from the source, the percentage reflected by the plate is obtained by dividing the numbers contained in the second column by those in the fourth, squaring, and multiplying by 100.

Table II gives a similar series of measurements with the same plate and light polarised perpendicularly to the plane of incidence.

Three other similar series of observations were made, the actual determinations being about as concordant as those contained in the tables. The four sets of observations, and their means, are given in Tables III and IV.

Table I.

63·0, 63·5, 63·0, 63·0							
	63·5	84·6, 83·7, 85·1, 83·0	84·1	....	30°	....	57·01
63·6, 64·0, 64·0, 63·9							
	63·8	80·8, 81·7, 81·7, 80·8	81·2	....	40°	....	61·73
64·5, 63·7, 64·0, 63·1							
	63·6	78·5, 78·8, 79·4, 78·1	78·7	....	50°	....	65·31
63·8, 62·5, 63·5, 63·9							
	63·6	77·1, 75·7, 77·7, 76·4	76·7	....	55°	....	68·76
64·0, 64·4, 64·3, 63·0							
	63·9	75·9, 75·1, 76·5, 76·3	75·9	....	60°	....	70·88
63·4, 64·2, 63·8, 64·2							
	63·8	73·3, 71·7, 73·4, 72·1	72·6	....	65°	....	77·22
63·5, 64·1, 64·0, 63·2							
	64·0	71·7, 71·1, 70·8, 70·1	70·9	....	70°	....	81·48
64·5, 64·0, 64·5, 64·4							
	64·1	70·3, 69·2, 70·4, 69·8	69·9	....	75°	....	84·09
64·0, 63·8, 64·0, 63·7							
	64·1	70·1, 70·4, 69·3, 69·0	69·7	....	80°	....	84·58
65·0, 64·7, 64·7, 63·1							

Table II.

79·2 77·3 78·8 77·1							
	77·7	112·0 110·0 112·0 109·0	110·7	....	30°	....	49·27
77·5 77·4 77·5 77·3							
	78·2	116·3 114·5 118·0 115·0	115·9	....	40°	....	45·53
80·0 78·6 79·3 78·3							
	78·8	124·5 124·0 124·0 123·1	123·9	....	50°	....	40·45
79·2 79·2 79·3 77·2							
	78·6	128·8 128·2 127·3 129·2	128·4	....	55°	....	37·47
79·2 78·6 79·0 77·5							
	80·0	136·5 132·8 135·7 131·9	134·2	....	60°	....	35·54
82·0 80·0 81 9 81·7							
	81·4	149·7 149·2 151·1 148·8	149·7	....	65°	....	29·57
81·5 81·8 81·7 80·5							
	81·7	163·8 161·1 162·3 157·7	161·2	....	70°	....	25·69
82·8 81·3 82·4 81·8							
	81·9	170·0 166·2 169·2 167·6	168·2	....	75°	....	23·71
80·6 81·9 82·5 82·2							
	82·7	161·0 160·8 163·0 160·4	161·3	....	80°	....	26·29
82·6 82·9 85·5 83·3							
	84·2	164·0 164·2 162·2 164·6	163·7	....	80°	....	26·46
85·5 84·1 86·0 84·0							

Table III.—Steel, with Light polarised in the plane of Incidence.

Angle of Incidence.	A.	B.	C.	D.	Mean.
30°	57·01	61·67	63·06	61·05	60·70
40	61·73	64·04	68·18	62·90	64·21
50	65·31	67·41	71·97	69·41	68·52
55	68·76	70·41			
60	70·88	74·55	77·96	74·31	74·42
65	77·22	76·02	81·40	74·83	77·37
70	81·48	80·77	85·22	81·57	82·26
75	84·09	84·92	90·32	84·71	86·01
80	84·58	86·34	91·55	89·01	87·87

Table IV.—Steel, with Light polarised perpendicularly to the Plane of Incidence.

Angle of Incidence.	A.	B.	C.	D.	Mean.
30°	49·27	50·53	53·67	47·28	50·19
40	45·53	45·39	49·79	44·40	46·28
50	40·45	41·24	43·47	38·78	40·98
55	37·47	37·34			
60	35·54	33·79	36·90	32·89	34·78
65	29·57	28·88	31·97	29·70	30·03
70	25·69	26·61	27·72	26·14	26·54
75	23·71	25·55	25·38	24·30	24·73
80	26·29	26·46	27·60	26·04	26·60

Similar measurements were made with the speculum metal mirror, and the results are given in Tables V and VI.

Table V.—Speculum Metal, with Light polarised in the Plane of Incidence.

Angle of Incidence.	A.	B.	C.	D.	Mean.
30°	64·58	64·09	63·37	66·18	64·55
40	67·76	68·22	65·14	69·86	67·74
50	72·65	72·23	69·04	71·90	71·45
60	76·63	78·65	77·57	77·95	77·70
65	79·65	79·68	79·44	81·26	80·01
70	83·09	81·25	84·94	83·90	83·29
75	82·94	84·20	86·93	88·01	85·52
80	87·52	86·78	90·96	89·72	88·74

Table VI.—Speculum metal, with Light polarised perpendicularly to the Plane of Incidence.

Angle of Incidence.	A.	B.	C.	D.	Mean.
30°	59·31	57·86	59·83	59·63	59·16
40	53·30	54·01	56·41	54·29	54·50
50	49·47	51·44	49·61	49·69	50·05
60	41·50	43·36	44·02	43·83	43·18
65	39·95	39·12	40·50	40·85	40·10
70	38·27	35·84	37·42	38·29	37·45
75	36·20	34·45	36·84	35·88	35·84
80	40·51	38·67	41·22	41·15	40·39

The principal incidences and azimuths for both plates were determined in the manner described in "Proc. Roy. Soc.," vol. 31, p. 486, and vol. 35, p. 32. Four observations were made in each position of the retarding plate, two with the principal section of the polarising Nicol on the right, and two with it on the left of the plane of incidence. The means of the several sets of eight observations are given in Table VII; the probable error of the mean result, calculated by the ordinary formula $0·6745\sqrt{\frac{\Sigma\Delta^2}{n(n-1)}}$ is also given.

Table VII.

	Principal incidence.	Mean. Prob. error.	Principal incidence.	Mean. Prob. error.
Steel..........	76° 05′	76° 20′ ± 5′	28° 31′	28° 29′ ± 1′.
	76 17		28 33	
	76 17		28 28	
	76 42		28 26	
Speculum metal..	74 48	75 31 ± 7′	33 28	33 02 ± 6′.
	75 23		33 09	
	76 07		32 32	
	75 38		32 40	
	—		—	
	75 39		33 23	
	75 32		33 01	

A beam of ordinary light being equivalent to two beams of half its intensity polarised at right angles to each other, the percentage of light reflected by the plates, if ordinary unpolarised light were incident upon them, is given by half the sum of the intensities of the light polarised in, and perpendicular to, the planes of incidence.

In Table VIII the numbers obtained in this way are given in the 2nd and 5th columns, the values as determined by the experiments described in the paper already referred to in the 3rd and 6th columns, and in the 4th and 7th columns the results calculated out, as described in a subsequent part of the present paper, from the measurements made with polarised light.

Table VIII.

	Steel.			Speculum metal.		
	Observations made with polarised light. $\frac{J^2+I^2}{2}$	Observations made with ordinary light. $\frac{rJ^2+I^2}{r+1}$		Observations made with polarised light. $\frac{J^2+I^2}{2}$	Observations made with ordinary light. $\frac{rJ^2+I^2}{r+1}$	
		Observed.	Calculated.		Observed.	Calculated.
30°	55·44	54·93	56·62	61·85	66·87	62·39
40	55·24	55·62	57·26	61·12	67·26	62·61
50	54·75	56·74	57·84	60·75	67·26	63·15
60	54·60	57·63	59·04	60·44	66·32	64·31
65	53·70	58·37	59·0	60·05	66·53	64·53
70	54·40	58·09	60·65	60·37	67·65	65·51
75	55·37	58·69	62·33	60·68	67·43	66·22
80	57·23	63·56	64·10	64·56	70·17	69·98

In addition to the actual numerical differences between the values, the two sets of observations appear to differ fundamentally, for whilst the numbers in the second and fifth columns diminish slightly, and then increase again, as the angle of incidence increases, those in the third and sixth increase with the angle, a result that, as was pointed out in the former paper, is not in accordance with either theory, or previous observations, and which, as was stated in the paper, if erroneous, must have been caused by some defect in the method employed, and therefore common to all the determinations.

Further consideration showed that such was really the case, and that the defect in the method was the one pointed out by Professor Stokes in the note appended by him to the paper ("Proc. Roy. Soc.," vol. 35, p. 39). As has already been stated, the apparent brightness of the paper varied with the polarisation of the incident light, or, in other words, the amount of light irregularly reflected, or diffused, by the paper was different for light polarised in, or perpendicularly to, the plane of incidence. The light being incident upon the paper at an angle of about 30°, whilst the line of sight formed an angle of

about 60° with the normal, no regularly reflected light could reach the observer.

The terminal faces of the polarising Nicol were perpendicular to its long diameter, and it was of a nearly circular section, so that the amount of light transmitted by the Nicol must have been very nearly, if not absolutely, the same in both positions.

Table IX gives observations made to determine the amount of illumination in both cases, the numbers in the first column being the distances at which the sliding lamp had to be placed when the light was polarised in the plane of incidence, and those in the second, when it was polarised in a plane at right angles to this; each determination being the mean of four observations.

Table IX.

	70·9	....	85·3
	70·0	....	90·4
	68·8	....	86·9
	70·0	....	88·8
Mean....	69·9	....	87·8

Calling the light diffused by the paper when the incident light was polarised in the plane of incidence 100, it would appear that when the light was polarised perpendicularly to that plane only about 63 per cent. of the light reached the observer.

The very considerable difference in the amount of light diffused in the two cases seems the more remarkable, as previous to the publication of the former paper the illuminated surfaces of the photometer were examined with a bi-quartz, and although they showed traces of polarisation it was only, apparently at least, to a very inconsiderable extent.

This experiment has been recently repeated, and with the same result; the lamp light diffused by the paper of the photometer showing hardly any, if any, traces of polarisation, whilst that reflected obliquely from the blackened surface of the board along which the lamp was arranged to slide, when examined in the same way, was seen to be strongly polarised.*

* [Of the light falling on the paper, a considerable part would be reflected at various depths before it had lost, through the various irregular reflections and refractions, all traces of its original polarisation, and consequently light derived from that which was originally polarised in the plane of incidence would be more copiously reflected than light derived from that which had been polarised in a perpendicular plane. But the light so reflected would have to make its way among the fibres of the paper, especially as the angle of emergence was considerable, and in so doing would be pretty well depolarised by the irregular reflections and refractions which it would have to undergo. This accounts for the circumstance

Owing to the unequal reflection of light polarised in and perpendicularly to the plane of incidence, the experimental results contained in the paper published in the "Proc. Roy. Soc." vol. 35, p. 26, cannot be the true values of the amount of light reflected by the mirrors.

The lamp light which was incident upon the mirrors, being equivalent to two beams of half its intensity polarised in and perpendicularly to the plane of incidence, and light polarised in and perpendicularly to this plane being unequally diffused by the paper, as well as unequally reflected by the mirrors, the observed intensity must, as Professor Stokes pointed out, be $\frac{rJ^2+I^2}{r+1}$, and not $\frac{J^2+I^2}{2}$.

Assuming the values of J^2 and I^2 found from the determinations with polarised light, and the value of r from the measurements contained in Table IX, the value of $\frac{rJ^2+I^2}{r+1}$ for the two mirrors at different incidences were calculated out,* and the numbers thus obtained are given in the fourth and seventh columns of Table VIII.

In the case of the steel mirror the observed and calculated numbers agree tolerably, as well, perhaps, as could have been expected, recollecting the nature of the determinations, but with the speculum metal mirror the results are discordant, the calculated results being in all cases too low. The observations with unpolarised light were made immediately after the mirror had been polished, whilst an interval of several months elapsed before those with polarised light were finished. Although the mirror was kept in a dry warm room, and in a closed case containing lime, its surface was usually found to be covered with a slight film; this was readily removed by rubbing it gently for a few seconds with a piece of wash-leather, and the surface then appeared perfectly bright.

After the conclusion of the experiments with polarised light, the photometer was rearranged in its original form, and three observations were made of the amount of unpolarised light reflected by the mirror at an angle of 30° in order to ascertain whether the reflective power

that the illumination was so different that was produced by light polarised in the two ways, even though the polarisation of the light coming from the paper was very feeble when the light incident was common light. I overlooked this when I proposed ("Proc. Roy. Soc.," vol. 35, p. 39) to measure r by measuring the polarisation of the light coming in this case from the paper, and regarded r as only "a little" greater than J on the strength of the author's assurance that the polarisation was so slight.—G. G. S.]

* The actual calculations were made with the equivalent formula $\frac{J^2+rI^2}{100+r}$, in which $r=63\cdot38$.

of the mirror had been diminished by the formation and removal of this film on several occasions.

	Light reflected, per cent.
	62·42
	63·04
	63·30
Mean......	62·92

The mean of these three observations agrees very fairly with the value deduced from the observations with polarised light, and it therefore seems probable that the differences between the numbers contained in the sixth and seventh columns of Table VIII are due to an alteration in the surface of the mirror, analogous possibly to the surface changes that Seebeck found with freshly polished transparent bodies.

The values obtained for the principal incidence, and principal azimuth, with the freshly polished mirror, and with the same mirror at the conclusion of the experiments with polarised light, differ but little from each other, as is shown in Table VII. The first four observations with the speculum metal mirror were made at the end of the experiments, and their means 75° 35′ and 33° 12′ agree very closely with the means of the last two observations 75° 29′ and 32° 57′ made with the freshly polished mirror.

The amount of light which, according to Cauchy's theory, ought to have been reflected by the mirrors was calculated out by the formulæ—

$$J^2=\frac{\theta^2+\cos^2 i-2\theta\cos\epsilon\cos i}{\theta^2+\cos^2 i+2\theta\cos\epsilon\cos i} \text{ and } I^2=\frac{\theta^2\cos^2 i+1-2\theta\cos\epsilon\cos i}{\theta^2\cos^2 i+1+2\theta\cos\epsilon\cos i},$$

and the results set forth in Tables X and XI.

Table X.—Amount of Light reflected by Steel Mirror.

	Observed.		Calculated.	
	J^2.	I^2.	J^2.	I^2.
30°	60·70	50·19	63·17	54·95
40	64·21	46·28	66·44	51·31
50	68·52	40·98	70·80	42·09
60	74·42	34·78	76·72	39·24
65	77·37	30·03	79·52	35·32
70	82·26	26·54	83·04	31·62
75	86·01	24·73	86·85	29·46
80	87·87	26·60	90·97	32·39

Table XI.—Amount of Light reflected by Speculum Metal Mirror.

	Observed.		Calculated.	
	J^2.	I^2.	J^2.	I^2.
30°	64·55	59·16	69·78	62·82
40	67·74	54·50	72·58	59·74
50	71·45	50·05	76·18	55·37
60	77·70	48·18	80·77	49·59
65	80·01	40·10	83·42	46·38
70	83·29	37·45	86·32	43·53
75	85·52	35·84	89·44	42·29
80	88·74	40·39	92·77	45·88

As far as the general character of the phenomena the agreement is complete and in accordance with the observations of M. Jamin, but the actual values of the observed intensities always fall short of the calculated intensities, the difference being least with the steel mirror.

The probable errors of the values of the principal incidences and azimuths having been ascertained, the theoretical intensities of the light reflected at an angle of 30° were calculated for the two values obtained by adding and subtracting these sums from the means. The probable errors of the photometric measurements for the same angle were also determined, and Table XII gives the values thus obtained.

Table XII.

Steel Mirror.

	Observed.		Calculated.	
	J^2.	I^2.	J^2.	I^2.
30°	61·57 ..	51·09	63·38 ..	55·17
	59·83 ..	49·29	62·97 ..	54·72

Speculum Metal Mirror.

30°	64·95 ..	59·60	70·24 ..	63·35
	64·15 ..	58·72	69·37 ..	62·37

These numbers seem to show that the differences between the calculated and observed results are not merely due to errors of observation, a conclusion that is rendered the more probable by the fact that the difference is always in the same direction.

The polish of the mirrors was examined at the end of the experiments by the method suggested by Professor Stokes, and described in

the paper already referred to; both the mirrors stood the test satisfactorily, the polish of the steel being very slightly the best.

These experiments appear to show that the generally received formulæ for metallic reflection are approximately correct, but that the actual intensity of the reflected light is always less than the theoretical intensity, and that therefore, unless this be due to defects in the metallic surfaces, the formulæ do not completely express the laws of metallic reflection. If, as appears to be the case, a change in the reflective power of a plate can occur without any change in the values of the principal incidence and azimuth, it is necessary to regard the formulæ as only approximately true, and there is additional reason for thinking that, as Professor Stokes has suggested, three constants are required to define a metal optically.

I hope hereafter to determine the amount of light reflected by films of silver chemically deposited on glass, and also to make some determinations of the amount of radiant energy reflected by metallic surfaces at various angles, the experiments of MM. de la Prevostaye and Desains on this point having been but few in number.

III. "Extracts from a Report on the Volcanic Eruption in Sunda Strait by Commander the Honourable F. C. P. VEREKER, H.M.S. 'Magpie,' dated Singapore, October 22, 1883." Communicated by Sir FREDERICK EVANS, K.C.B., F.R.S. Received December 15, 1883.

[PLATES 2, 3.]

* * * On the 18th instant I entered Sunda Strait, passing east of Thwart-way Island. This island had been reported to be split by the eruption into several portions. This is incorrect.

The island is intersected by low valleys in several places, these being covered with tall trees did not show so prominently formerly as they do now. The whole of the vegetation having been swept away by the tidal wave the island at a short distance off is apparently divided, the low necks joining the higher portions being only visible on close approach.

The surface of the Strait in this neighbourhood is covered with extensive fields of floating pumice stone, often in one to two foot cubes, through which the ship easily forced her way.

* * * * * *

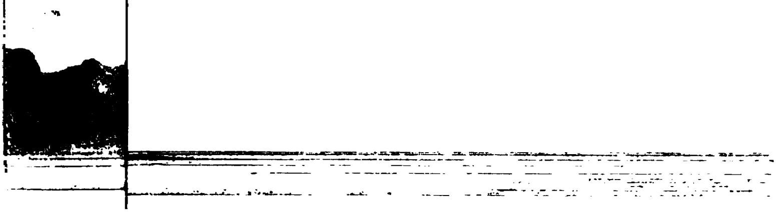
I inclose sketches which I trust will convey the general appearance better than a written description. The whole of the neighbourhood is covered with greenish-yellow mud, and all traces of vegetation are everywhere destroyed.

NDIAN

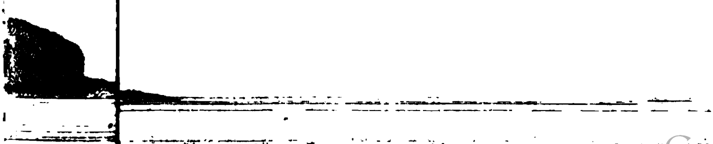
ruption

Sebooko Island.

A



B



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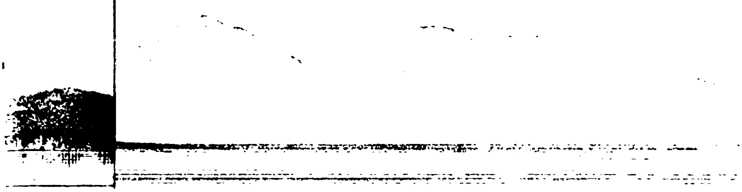


D

The Islands are totally devoid of vegetation, everywhere scored and seamed by the lava streams, and covered with a deposit of greenish brown mud.

n the yea

Emma.

West Newman & Co. lith.

I communicated personally with the captain of the Netherlands frigate "Queen Emma" stationed on the spot, and was informed by him that the changes are considerably more extensive than was at first thought, and that Verlaten Island is still in a state of activity as well as Krakatoa itself.

From observation he thinks that another eruption is impending, but that Verlaten Island will be the centre of disturbance.

The Netherlands Government vessel "Hydrograaf" obtained a sounding of 100 fathoms without reaching bottom, in the centre of the group and off the cliff falling from Krakatoa Peak.

The two new islands are low mud and pumice banks, their configuration is continually altering, and I was informed that they are gradually subsiding.

* * * * * *

It is still impossible to examine Lampong Bay, but the pumice stone is now beginning to float out.

The light on Fourth Point (Java) has been temporarily replaced by one of the 6th order, visible five miles, but beside this there are no signs of life on the Java shore. The whole coast is covered with the *débris* of trees, &c., demolished by the earthquake sea-wave, and over all lies a thick incrustation of volcanic mud.

During the height of the eruption a terrific whirlwind and a fierce south-westerly gale, apparently local, was experienced.

* * * * * *

IV. Report from H.B.M. Consul at Batavia, inclosing Extract relating to the Volcanic Outbursts in the Sunda Strait, from the Logbook of the Steam-ship "Governor-General Loudon." Communicated by R. H. Scott, F.R.S. Received December 4, 1883.

Sunday, 26th August.—Left the roadstead, Batavia, at 8.10 A.M., and steered through the inside channel. At 9.30 A.M., steering between the islands Great Kombuis (or Lantjang) and Pulo Lakki (or Mometer or Cannibal Island), sighted the Kombuis red buoy due north, while at 10 A.M. the white Cannibal buoy lay due south. Rounded Point St. Nicholas, and taking our bearings from the land proceeded through Sunda Strait to the roadstead of Anjer, where we anchored at 2 P.M.

At Anjer we took on board 111 passengers, coolies and women bound for Sibogha, and left Anjer roadstead again at 2.45 P.M., and taking our bearings from the land we ran past Pulo Soengjan, or "Right in the Fairway" Island, past Hog Point and Lampong Bay, and then discovered that the island of Krakatau was casting forth enormous

columns of smoke. At 5 P.M. Pulo Tiga Island lay about half an English mile to the west of us. Were steering then N.W. ¼ N. (time bearing). At 6.15 P.M. the southernmost of the Chandon Islands was bearing north-east.

Laid the course next for the roadstead of Telok Betong, which we reached at 7.30 P.M., and where we anchored in six fathoms of water with thirty fathoms shackle outside the hawse-pipe.

From 6 o'clock we had rain of ashes and small bits of stone, and there was a stiff breeze from the N.W. and W.N.W.

Monday, August 27th.—Finding that at midnight on the evening of our arrival there was still no boat come off to us from the shore, and as the weather was now much calmer, I sent the first mate in the gig with a crew of six men to find out what was the reason of this. About 1 A.M. he returned, and stated that it had been impossible to land on account of the heavy current and surf; also that the harbour pier-head stood partly under water.

The Government steamer "Berouw," which lay anchored near the pier-head, hailed the mate as he was returning on board, and the people on board her then stated to him that it was impossible to land anywhere, and that a boat which had put off from the shore had already been wrecked. That by 6 P.M. on Sunday evening it had already begun to be stormy, and that the stormy weather had been accompanied by a current which swept round and round (apparently a sort of whirlpool). When the mate had come on board, we resolved to await daylight before taking any further steps; however, for the sake of security, we steamed several ships' lengths outwards, because the sound of a ship's bell which seemed to be approaching us made us suspect that the ship must be adrift, and wishing therefore to avoid a collision we re-anchored in nine fathoms with thirty fathoms shackle outside the hawse-pipe. We kept the ordinary sea-watch, and afterwards heard nothing more of the bell. When day broke, it appeared to us to be still a matter of danger to send a boat ashore; and we also discovered that a revenue cutter was foul of a sailing-vessel which lay in the roadstead, and that the "Berouw" lay very close in shore. Later we found that the "Berouw" was stranded. However, owing to the violent winds and currents, we did not dare to send a boat to her assistance.

About 7 P.M. we saw some very high seas, presumably an upheaval of the sea, approaching us up the roadstead. These seas poured themselves out upon the shore and flowed inland, so that we presumed that the inhabitants who dwelt near the shore must be drowned. The lighthouse was altogether carried away, and the "Berouw" then lay high upon the shore among the cocoanut trees.* Also the revenue

* And as far as ascertained since, every soul on board was killed. The steamer now lies two miles inland.

cutter lay aground, and some native boats which had been lying in the neighbourhood at anchor were no more to be seen.

Since it was very dangerous to stay where we were, and since if we stayed we could render no assistance, we concluded to proceed to Anjer under steam, and there to give information of what had taken place, weighed anchor at 7.30 A.M., and following the direction of the bay steered thereupon southwards. At 10 A.M. we were obliged to come to anchor in the bay in 15 fathoms of water because the ash rain kept continually growing thicker and thicker, and pumice-stone also began to be rained, of which some pieces were several inches thick. The air grew steadily darker and darker, and at 10.30 A.M. we were in total darkness, just the same as on a very dark night. The wind was from the westward, and began to increase till it reached the force of a hurricane. So we let down both anchors and kept the screw turning slowly at half speed in order to ride over the terribly high seas* which kept suddenly striking us presumably in consequence of a "sea quake," and made us dread being buried under them.

Awnings and curtains from forward right up to the mainmast, three-boat covers, and the uppermost awning of the quarter deck were blown away in a moment. Some objects on deck which had been lashed got loose and were carried overboard; the upper deck hatchways and those on the main deck were closed tightly, and the passengers for the most part were sent below. Heavy storms. The lightning struck the mainmast conductor six or seven times, but did no damage. The rain of pumice-stones changed to a violent mud rain, and this mud rain was so heavy that in the space of ten minutes the mud lay half a foot deep.

Kept steaming with the head of the ship as far as possible seawards for half an hour when the sea began to abate, and at noon the wind dropped away entirely. Then we stopped the engine. The darkness however remained as before, as did also the mud rain. The barometer at that time stood again at 763·25 millims. Sounded the pumps. No water. Let the crew and also such passengers as were on deck work at throwing the mud overboard. At 2 P.M. the barometer was 763·30. The mud rain changed into a light ash rain.

The darkness remained the same until the following morning at 4 A.M. At daybreak began to get the chain clear and weigh the anchor; got under steam at 6.30 A.M.; made out then Tims Island, bearing S. by W., and Pulo Soengal Island, bearing S.W. ½ W. Held on our course for Pulo Tiga Island and fell in with much ash and drift-wood.

When we got about two English miles off Pulo Tiga, it appeared that a connexion had been formed between the islands just mentioned

* Or, as he says elsewhere, "Squalls, storms, and seas as high as the heavens."

stretching to Seboekoe Island, and thence to the mainland. Whether this connexion was formed out of solid ground or only out of pumice-stone and trunks of trees is not known. What is certain is, that at the distance at which we then lay, it looked exceedingly like solid ground, and so we thereupon turned back in order to look for another passage. The very same phenomenon as had revealed itself near Pulo Tiga was discovered also between Tims Island, Seboekoe, and Pulo Soenchal, and we, therefore, resolved to make our way out through the Strait of Lagoendie. This channel we found relatively clear, that is, in comparison with the other part of the bay. Having got outside we discovered that here also we were entirely shut in; so steamed very slowly, stopping every now and then close by the so-called layer, and made it out at last to be floating pumice-stone, through noticing that the layer was heaved up and down by the motion of the surf.

Now steamed somewhat faster, and when we got into the middle of the layer before referred to found it to be 7 or 8 feet thick. It took us ten minutes to get clear of it, and then we held our course south of Krakatau, the serang* being in the fore-yard, a man on the look out forward, and the captain and first mate on the bridge.

Wednesday, August 28th, 1883.—As we steamed past Krakatau we noticed that the middle of the island had disappeared, and that no smoke was to be seen in any direction. However, when we got east of Krakatau† we discovered that between that island and Sebisie a reef had formed, and that various craters planted on that reef were now and then sending columns of smoke on high. As we neared the coast of Java we observed that here, too, everything had been laid desolate.

We also perceived that the lighthouse on Java's Fourth Point was entirely washed away; nothing remaining except a stump some feet high.

At 4 o'clock reached Anjer roadstead, and although there was nothing more to be seen of the place itself the captain and the first engineer went ashore to learn what information was to be obtained.

Ashore we met the Resident of Bantam, and concluded to return straight on board in order to convey his Honour to the roadstead of Bantam, and this because the Resident assured us that it was of very great importance in the public interest that such should be done.

We left Anjer Roads at 10 minutes to 5 o'clock, steamed round St. Nicholas' Point, and went on taking our bearings for the Island of Pulo Pundjang, and steering into Bantam Bay.

* Serang is a sort of boatswain.

† In another place he says also that, "Also half of the island 'Right in the Fairway' had disappeared, and what is left of it has been broken into fragments with open spaces between them."

We arrived at our destination at 6.50 P.M.; by request of the Resident, put him on board a ship which lay in the roads there, and after having done this at 7.30 we got under steam again and made for Kroe (Benkoelen district).

T. H. LINDERMAN,
Master of the "Governor-General Loudon."

Additional Notes.

The telegraph buoys in the neighbourhood of Anjer may possibly have been somewhat shifted, but they were found not to have been carried away.

The town of Tjiringen (Java) has been destroyed.

At Anjer, besides many natives who are supposed to have perished, the Assistant Resident, the harbour master, and nearly all the other Europeans were destroyed.

The lighthouses at Java First Point and Flat Cape (Sumatra) are still standing.

At Padang from the afternoon of the 26th of August for the succeeding twenty-four hours re-echoing sounds made themselves heard like the noise of distant and heavy cannonading.

The air now and then was red-coloured, while during the afternoon of the 27th thirteen bores or tidal waves rushed up the harbour, the third and highest of these waves rising to about $\frac{1}{2}$th of a metre below the crown of the quay on which the harbour master's office stands.

The strength of these waves varied for the most part between that of a four-mile and of a six-mile current, but the third wave was estimated to be running at a speed of about twelve miles.

Beginning at 12.50 P.M. they recurred at intervals till nearly midnight. The average pause between the flow and the ebb of these waves was about four seconds.

It will be seen from the foregoing that three lighthouses are known to have been destroyed, viz., the harbour lights at Anjer and Teluk Betong, which were small, and the big lighthouse on Java Fourth Point.

It is worthy of note also that on the morning of the 27th of August before daybreak the master of the British steam-ship "Devonhurst," which was then off the North Coast of Acheen steering for Olehleh, was woke by a shock which led him to think that the steamer had stranded.

He rushed on deck to learn what had happened, but found his vessel in deep water in her usual course.

One or two superstitious ideas which revealed themselves among the native population in consequence of the disaster are not unworthy

of notice. Thus on board the "Governor-General Loudon" the mud rain* which covered the masts, rigging, and decks, was phosphorescent, and on the rigging presented the appearance of St. Elmo's fire. The natives engaged themselves busily in putting this phosphorescent light out with their hands, and were so intent on this occupation that the stokers left the engine-rooms for this purpose, so that the European engineers were left to drive the machinery for themselves. The natives pleaded that if this phosphorescent light, or any portion of it, found its way below a hole would burst in the ship: not that they feared the ship taking fire, but they thought the light was the work of evil spirits, and that if the ill-omened light found its way below the evil spirits would triumph in their design to scuttle the ship.

Recent telegrams from the scene of the disaster describe the native population as hostile, and inclined to attribute the catastrophe to the vengeance of heaven pursuing the Dutch for their conduct in Acheen. Such Europeans as escaped the eruption are now reported to be in danger of being murdered by the natives.

These accounts are, however, possibly exaggerated. The Dutch officials make light of them, and deny that there is any ground for political disquietude.

Krakatau Island before the eruption was 822 metres high.

Pulo Bessi, the neighbouring island, was 849 metres high.

The earthquake waves which deluged Anjer and the neighbouring coasts were two in number; the second was higher than the first, but the first was reckoned to have been 13 feet high when it touched the land.

There was an hour's interval between these two waves.

The shock of these waves (or of one of them) drove the ships and the dry-dock in Onrust Island, near Batavia, from their anchorage.

The earthquake wave was still as much as 6 feet high when it struck the shore at a place called Sembilangan Laoet, more than 2 English miles to the eastward of Batavia, and there it killed at least seven people.

At Tjiringen, on the Sunda Straits, the earthquake waves are reckoned to have killed 10,000 people, and it is also known that these waves have flooded the coasts further southward, as far as Paniembang river, in Pepper Bay (how much further has not yet been ascertained). To the eastward a reference to the map will show that the promontory ending in St. Nicholas Point, and which shelters Bantam Bay, is mountainous, containing two peaks, 640 and 634 metres high respectively.

Hence, as far as I can gather from the recent reports, the earthquake waves appear not to have done much damage to Bantam Bay,

* It is supposed to have been raining violently at the time; the rain mingled with the ash and formed the "mud."

but to have swept past St. Nicholas Point eastwards, with a slight direction south, and to have struck Tanara, a town near the coast on the river which separates the provinces of Serang and Batavia. In the Tanara district 704 corpses have already been found. The wave did immense damage at Kramat, a coast village about 2 miles eastward of Tanara, and killed many people there. It struck the land on the east side of the bay in which Batavia lays with a column of water, which was still 6 feet high; and somewhere about this district the force of the flood seems, as far as is at present known, to have expended itself.

The first eruption on Krakatau Island took place about the 23rd of last May, and continued at intervals for a day or two. It is not without interest to note that Captain Gibson, commanding the steam-dredger "Crocodile," on its way to Australia, was, on August 21st, in latitude 7° 30′ S. and longitude 90° 30′ E. There he found his vessel amid great quantities of floating pumice-stone, some of which he brought on board and has preserved. He attributed the presence of this pumice-stone to some volcanic eruption, which he supposed had taken place among the Eastern islands.

The current at that time was setting westward, at the rate of about a knot and a half per hour. There were barnacles and shells on some of the pumice-stone, showing that it must have been for some time in the water. Most probably this was the *débris* from the May eruptions on Krakatau.

There may possibly be questions connected with the migration of animal or vegetable life, or with the duration and drift of oceanic currents on which the above incident may have an important bearing, or perhaps *would* have if it were possible to trace what ultimately became of the pumice-stone which was met with by the "Crocodile" travelling steadily westward, and which had already travelled so far.

In the neighbourhood of Anjer coral rocks more than six tons in weight were rolled by the sea far inland.

The corpses are so battered that it is often impossible to recognise whether the man was a Chinese or a Malay.

On the southern coasts of Java the flood waves rolled ashore, and did damage as far to the eastward as the province of Banjoemas.

In some places where the ash rain has choked the wells there is a scarcity of water.

(Signed) H. G. KENNEDY,
Her Majesty's Consul.

September 13, 1883.

V. "Experimental Researches on the Electric Discharge with the Chloride of Silver Battery." By WARREN DE LA RUE, M.A., D.C.L., Ph.D., F.R.S., and HUGO MÜLLER, Ph.D., F.R.S. Received December 21, 1883.

Plasticity and Viscosity of Strata.

During our experiments we have often been struck by the evident plasticity of strata whose form at times becomes modified when they meet with an obstacle or are influenced by other causes, as, for example, the crossing of other strata produced by a separate discharge;* and we have stated in Part IV of our researches that "one cannot but be impressed from this (an experiment therein spoken of) and others before,† and herein described, by the apparent plasticity of the aggregate assemblage of molecules which constitute a stratum." In all probability the molecules are being continuously thrown off and are replaced by others which become controlled and held in position by the same balance of forces as those they replace.

One of our tubes, No. 9, with a residual hydrogen vacuum, has a diaphragm in the centre $\frac{1}{4}$ of an inch, 0·63 centim., thick, through the centre of which there is a hole $\frac{1}{4}$ of an inch, 0·63 centim., in diameter. To the end of the tube is attached a potash absorption chamber, the heating and cooling of which causes a change in the number of strata; when the number of strata increases they approach closer and closer to the diaphragm, and occasionally one threads itself through it, as if squeezed through, and its form is gradually changed thereby; when by a change in the temperature of the absorption chamber, the number of strata becomes less, the stratum which had been forced through the hole in the diaphragm returns through it, its form becoming modified to enable it to do so.

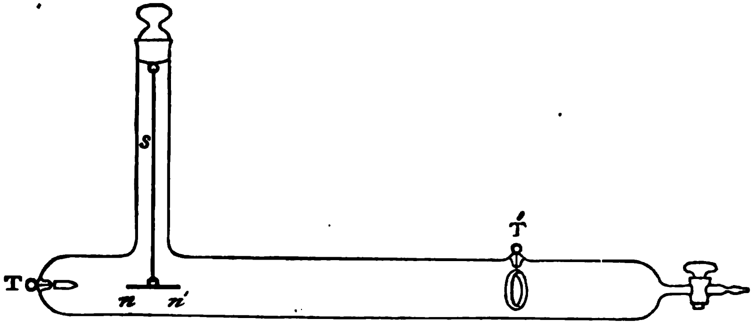
A tube, No. 368, fig. 1, with a hydrogen residue gives evidence of the viscosity of a stratum.

The tube is 14·5 inches, 36·8 centims., long, and 1·5 inches, 3·8 centims., in diameter, 9·75 inches, 24·8 centims., distant between the terminals, a point T and a ring T′. At right angles a tube of smaller diameter is attached 5 inches, 12·7 centims., long; in this tube is a stopper having a loop underneath from which is suspended by two silk fibres, *s*, a piece of decarbonised iron, *n n′*, $\frac{5}{8}$ inch, 1·6 centims., long, and 0·026 inch, 0·66 millim., in diameter. The stopper when greased turns quite smoothly, and by turning it the needle can be easily placed in any direction with regard to the tube.

* "Phil. Trans.," Part II, vol. 174, p. 398. Separate copy, p. 220.

† "Phil. Trans.," Part I, vol. 171, p. 257. Separate copy, p. 192.

FIG. 1.



In the first place the tube was placed in the magnetic meridian, and the needle of iron wire, *n n'*, in the same direction; tested by means of a very small magnet, both ends were equally attracted, showing that the needle had been thoroughly decarbonised; this was done by heating it to redness for many hours in peroxide of iron, prepared by burning its oxalate.

The discharge was in the first instance passed from the ring to the point, so that the needle was in the dark space; no magnetism was developed in the needle, which would have been the case if the discharge had had a spiral motion as we have often observed and described to be sometimes the case.* It was with the object of ascertaining this fact that the apparatus had been made.

FIG. 2.

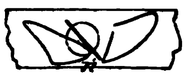
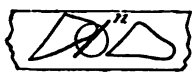


FIG. 3.



The needle was now placed at right angles to the tube, and the point made positive; after a few trials at different exhausts a beautiful tongue-shaped stratification was obtained, and it was then possible to make the apex of a stratum impinge on one or the other end of the needle, figs. 2 and 3, on whichever end the stratum touched, the needle was pushed away from it, showing clearly that the balance of forces which hold together the molecules composing a stratum are sufficient to render it viscous, and unyielding to a small resistance. With disk or saucer-shaped strata the whole length of the needle being touched at the same time, it had no tendency to turn it on its axis, but became agitated when touched by strata.

* "Phil. Trans.," Part I, vol. 169, pp. 250–253, 255–263, 265.

January 17, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On a New Method of Generating Electricity." By J. A. KENDALL, F.I.C., F.C.S. Communicated by Professor G. G. STOKES, Sec. R.S. Received September 30, and December 28, 1883.

In 1863 Deville and Troost announced their discovery that certain metals were permeable by hydrogen at a red heat. This discovery, as is well known, was verified by Graham, who made extended researches on the subject.

About three years ago it occurred to the author that a red-hot platinum plate, through which hydrogen was passing, might be made to serve as an element of a galvanic combination, and early in 1881 some experiments were tried with this object.

These were continued from time to time up to the present, and in this paper it is proposed to give some account of the experiments and their results. The subject appears to the author to require much more extended researches in several directions than he has been able to make, but it is hoped that by giving an account of the researches hitherto made, the points which require further investigation will suggest themselves to physicists and chemists.

In the earlier experiments the author constructed small tubes of platinum foil. These were sealed up at one end by the oxyhydrogen blowpipe, and to the open ends glass tubes were fused. Platinum conducting wires were fastened to the tubes. By means of the glass tubes gases could be conveyed to the interior of the platinum tubes.

A platinum crucible was used as the other element of the cell, it had a platinum conducting wire attached, and was supported over a Bunsen burner. A small platinum foil tube was then held in the centre of the crucible, and the cell was completed by putting the transmitting medium into the crucible. After unsuccessful trials with alkaline nitrates, &c., glacial phosphoric acid was selected. Some of

this substance was put into the crucible and fused so as to nearly immerse the platinum foil tube.

On connecting the two wires with a galvanometer no deflection was observed when the crucible and contents were heated to redness in the oxidising flame of the Bunsen burner. When, however, hydrogen gas was supplied to the inner platinum tube, an immediate production of electricity was perceived. The tube of platinum foil containing hydrogen was seen to correspond to the zinc element in an ordinary galvanic cell.

This experiment being verified other substances were tried instead of phosphoric acid.

Sulphuric acid nearly at its boiling point gave a slight current.

Chloride of sodium in the fused state gave a very good result. Then the chlorides of potassium, calcium, and the alkaline earth metals were tried with similar results.

As might be expected the production of electricity increased with the temperature.

It was, however, soon found that the production of the current was stopped if the flame of the Bunsen burner did not ensure perfect combustion on the exterior of the platinum crucible.

Experiments showed that if a reducing flame was applied to the crucible while the small tube contained air, then a current of electricity in the reverse direction was obtained.

Tubes of palladium foil substituted for the platinum tubes gave similar results.

After a number of trials with tubes of platinum foil, an apparatus was constructed of two platinum tubes closed at one end.

The inner tube was 4 inches long and $\frac{3}{8}$ inch internal diameter, while the outer tube was $3\frac{5}{8}$ inches long, and $\frac{5}{8}$ inch inside diameter. The thickness of metal was $\frac{1}{100}$ inch in both tubes.

The two tubes when used for these experiments were arranged upright in a small Fletcher's gas furnace, the inner tube being supported at any desired height in the centre of the larger tube and connected with a supply of hydrogen.

Numerous experiments were made with this apparatus. The temperature could be easily regulated from a dull red heat to a white heat, and various saline substances could be tried as media.

The fused sulphates, carbonates, and nitrates were found to be unsuitable for the production of the current. The results obtained with fused chlorides, &c., showed that the hydrogen not only passed through the metal of the inner tube, but also through the fused saline medium and then through the outer platinum tube.

In recent experiments with this apparatus it appears that when a good transmitting substance is used between the tubes, and when about 3·3 square inches of the inner tube are in contact with the

medium, the amount of hydrogen gas which passes through the metal at a nearly white heat is about 0·7 cub. centim. per minute. This volume of course refers to hydrogen at ordinary temperatures.

The use of this apparatus led to the discovery of a large number of substances which would serve as media by allowing the transmission of hydrogen.

The list of saline bodies was extended to the bromides, iodides, and fluorides of the alkaline and earth metals, but the most important discovery in this direction was that vitreous bodies such as glass, and even vitrified bodies as porcelain and earthenware, acted as media when at a red heat.

Attention was then directed to the use of vitreous media for several reasons.

In experiments with fused saline bodies the use of common metals was precluded owing to their being corroded by fused salts, and although iron is known to be permeable to hydrogen at a red heat, yet its oxidisable qualities prevented any satisfactory results being obtained when it was substituted for platinum.

However, when vitreous matters were used instead of fused salts, it became possible to use other metals for these experiments.

A number of trials were made by taking tubes of fusible soda glass. A small tube of the metal to be tested was then placed inside the glass, and while passing a slight current of coal-gas to prevent oxidation of the metal, the glass was carefully fused on to the metal. The external surface of the glass while soft was then coated with thin platinum foil or with other metals.

On connecting the inner and outer coatings with a galvanometer, passing hydrogen through the tube, and then heating it to redness, the usual current of electricity was produced.

The quantity of electricity generated both with these cells and with the platinum tubes was in proportion to the surface heated.

The most powerful effects were obtained when the metallic coating was in the pulverulent form. Spongy platinum, for example, when made to adhere closely to the glass gave a strong current with hydrogen.

When using thin metallic plates in the interior of the tubes it was found necessary to employ very thin platinum foil on the exterior, as the hydrogen otherwise accumulated to some extent on the inner plate, thus spoiling the cell.

A good result might often be got by painting the external surface of the glass with an alcoholic solution of platinic chloride, and then igniting. By this means a very thin film of metallic platinum was left on the glass, and by means of a spiral of platinum wire also put round the glass, sufficient conduction was obtained.

As glass exerts only a slight action on metallic iron at a red heat,

thin sheet iron ($\frac{1}{100}$ inch) was used in numérous experiments, but this does not make a perfect arrangement when glass containing alkali is used, as the alkali metal appears to be liberated at a very strong heat.

A number of metals were tried either in the form of sheet or as a powdery deposit.

This latter form might frequently be obtained by coating the interior of the glass tube with the oxide of the metal to be tried, and then reducing the metal by hydrogen or coal-gas upon the surface of the glass.

The following metals were tried and found to transmit hydrogen and cause the production of electricity :—

Platinum.	Molybdenum.
Palladium.	Copper.
Gold.	Silver.
Iron.	
Nickel.	

The relative transmitting powers were not, however, ascertained. There can be little doubt that the property of transmitting hydrogen at a red heat belongs to most, if not all, metallic bodies.

In the course of the experiments it was observed that the glass used was practically a non-conductor of electricity from one or two galvanic cells when it was heated to redness in an oxidising flame.

When, however, hydrogen was supplied to the glass either inside or outside of the tube it at once became a good conductor of the current.

It was found necessary to avoid using glass containing metallic oxides reducible by hydrogen, as these oxides by reacting upon the hydrogen on the surface of the transmitting plate cause frothing of the glass, thus destroying that absolute contact between the metal and the glass which is required for the production of the electric current.

Experiments were made with tubes of Berlin porcelain, and satisfactory results were obtained. It was found convenient to cover the surface of the tube both inside and out with melted glass and then to carefully lay platinum foil upon the glass, so as to get as perfect an adherence as possible. It appears to be best to have a much thicker metallic plate on the inner side of the cell than on the outside. The author has not determined the most advantageous conditions precisely as yet. Cells were also constructed of clay, containing a percentage of glass, porcelain of various kinds, &c.

The amount of hydrogen transmitted in a given time through the arrangements described varies greatly according to the nature of the medium and the nature of the metallic layer.

With pulverulent metals and a medium of soft glass the rate of transmission of hydrogen may be as high as 0·6 cub. centim. per square inch per minute at a full red heat. With Berlin porcelain tubes, however, the transmission does not usually exceed 0·2 cub. centim. per square inch per minute, even at a white heat, while at a red heat the rate is much lower. With platinum tubes of $\frac{1}{100}$ inch thick the transmission may be from 0·1 cub. centim. to 0·2 cub. centim. per square inch according to the temperature.

The electromotive force of the new cells varies according to the media used, and this subject will of course require further investigation. It was found, however, that the platinum tube cell gave, with borate of lime at a nearly white heat, an electromotive force=0·36 of a Daniell, whilst a cell constructed of Berlin porcelain tube $\frac{1}{12}$ inch thick gave an electromotive force=0·7 of a Daniell when worked at a nearly white heat.

Although many gaseous mixtures containing free hydrogen will serve to produce the electrical reactions, yet experiments with carbon monoxide have given no similar result in conjunction either with iron or platinum plates.

In conclusion, the author has to thank Mr. S. A. Sadler, of Middlesbrough, for having put at his disposal various facilities for prosecuting his later experiments.

Received December 28, 1883.

Before describing further researches which the author has made on the subject of electrical currents produced by hydrogen, it may be well to mention that the galvanometer generally used in the experiments yet to be described, as well as in the former experiments with metal tubes, &c., is one adapted for rather strong currents and it has very slight resistance. It has been graduated by means of a voltameter.

As these experiments must be regarded more as qualitative than quantitative, it will perhaps be sufficient to give four points of deflection with the corresponding liberation of hydrogen in a voltameter.

Deflection of galvanometer.	Liberation of hydrogen per minute.
10°	0·07 c.c.
20	0·21 „
30	0·60 „
40	1·35 „

The above figures also show, in a roughly approximate way, the amount of hydrogen which must be supplied to a cell of the new construction in order to give the deflection indicated.

In continuing his researches the author has found that strongly

heated hydrogen may give rise to electrical currents under a variety of circumstances.

Small cells were made by nearly covering short wires or rods of metal with melted glass. The glass was then covered with platinum foil and the two metals were connected by wires with the galvanometer. On heating a cell of this construction in an oxidising flame an electrical current was almost invariably produced, due to the withdrawal of hydrogen from the inner wire or rod. When the current diminished in force a reducing flame, containing free hydrogen, was applied to the cell. This immediately caused an energetic reverse current, accompanied by the re-absorption of hydrogen by the inner metal. Then with an oxidising flame the original effect could be produced. These results were obtained with wires of platinum, nickel, iron, and copper. In working with cells of this description made with iron rod, it was found that a current of electricity of long duration could be produced by the oxidising flame. This result appears to be due to the continuous absorption of hydrogen liberated from aqueous vapour by that portion of the iron which was not covered by glass.

When the entire surface of the iron was covered by glass (with the exception of the conducting wire, which was away from the heat) then the deflection of the galvanometer gradually came to zero when the cell was heated in an oxidising flame.

Recently experiments have been made with a differently arranged apparatus, as follows:—

A platinum tube, 5½ inches long and 1 inch diameter, was set upright in a Fletcher's gas furnace and nearly filled with a fusible glass composed of the diborates of lime and magnesia. This apparatus being heated to bright redness, a plate of platinum, 2 inches long and 0·6 inch wide, suspended by a platinum wire, was immersed in the fluid glass. The platinum tube and the plate being connected with the galvanometer, the phenomena of alternate electric currents could be produced with great facility by altering the nature of the flame in the furnace. When the platinum tube was surrounded with a visible pale flame there was an electrical current from the tube to the plate until the plate was apparently saturated with hydrogen. When more air was supplied to the furnace, so as to cause more perfect combustion, the needle of the galvanometer was violently reversed. The deflection produced on the galvanometer by either the "normal" or "reverse" current was at first 18° or 20°, and it fell to nearly zero within ten or fifteen minutes.

These effects could of course be repeated as often as required.

It appears quite plain that the hydrogen in flames has a powerful molecular or atomic action.

If glass be fused in a large platinum crucible heated by flame as in a Fletcher's furnace, bubbles of hydrogen may be observed forming

and rising from the sides of the crucible, especially at the hottest parts.

If a platinum tube like that used for the experiments with the suspended plate be somewhat cooled while nearly full of melted glass, so that the glass becomes very viscous, then by applying a flame containing free hydrogen to any spot on the lower part of the tube the latter may easily be burst by the bubble of hydrogen which is formed on the inside of the tube.

Experiments have also been tried in which the hydrogen coming through a cell was removed by means of a Sprengel pump. One experiment may be described:—A platinum tube $2\frac{3}{4}$ inches long and $\frac{3}{8}$ inch diameter, closed at one end, was soldered to a strong iron tube and fixed vertically. The platinum tube was immersed for $2\frac{1}{4}$ inches in fused glass contained in a platinum cell. This latter was $2\frac{3}{4}$ inches deep and 1 inch diameter.

The two platinum tubes were connected by platinum wires with the galvanometer, and the iron tube was connected with a Sprengel pump.

The cell being heated to bright redness in an oxidising flame, a good vacuum was produced by the Sprengel pump. Then, while no bubbles of gas came down the fall tube of the pump, the galvanometer showed no deflection.

The cell was then heated by a reducing flame. The galvanometer soon gave a steady deflection of 15°, and bubbles of hydrogen came down the fall tube of the pump. The experiment was continued for half an hour, and during fifteen minutes the hydrogen coming down the fall tube was collected. It measured 1·33 cub. centims.

From occasional experiments with several vitreous mixtures the conclusion formerly arrived at by the author regarding their electric conductivity is confirmed, viz., that these fused vitreous matters do not conduct electricity of low tension unless hydrogen be present. When working with large cells it is, of course, difficult to avoid the presence of hydrogen if the cell be heated by flame.

It appeared desirable to try whether any hydrogen could be made to pass through the walls of a porcelain tube either under the influence of oxygen or by means of a vacuum.

A glazed Berlin porcelain tube 20 inches long and $\frac{1}{2}$ inch diameter was sealed up at one end and connected with a Sprengel pump. The closed end of the tube exposing a surface of 4 square inches was heated to whiteness in the gas furnace, but no hydrogen could be drawn by the Sprengel pump when the porcelain tube was heated in a reducing flame.

After this the porcelain tube was filled with hydrogen, and the same part as before was heated in an oxidising flame, but no loss of hydrogen from the tube could be perceived during half an hour.

Somewhat similar experiments have also been made with glass tubes with negative results.

The author has also made a few experiments to ascertain the influence of a voltaic current in increasing or diminishing the flow of hydrogen through the medium, but so much depends upon the structure of the metallic surfaces in contact with the medium and their relative sizes, as well as upon the electromotive force, &c., of the battery used, that this subject would probably require somewhat elaborate researches.

The author, however, hopes to make further investigations into the nature of the movements of hydrogen produced in vitreous matters and in metals.

II. "On the Electrolysis of Dilute Sulphuric Acid and other Hydrated Salts." By J. H. GLADSTONE, Ph.D., F.R.S., and ALFRED TRIBE, Lecturer on Chemistry in Dulwich College. Received December 20, 1883.

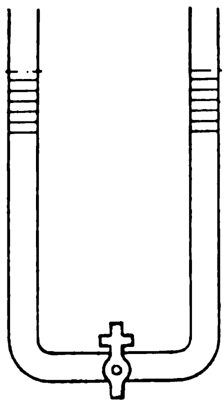
On the 1st of March last a communication was presented to the Royal Society by Professor Frankland, in which, among other things, the reactions we had described as taking place in the charging and discharging of secondary batteries were confirmed. The author expressed these reactions, however, by formulæ founded on the electrolysis, not of H_2SO_4, but of hexabasic sulphuric acid, H_6SO_6, in accordance with the views of Bourgoin.

The point of difference is a small one, but it led us to look into the papers of Bourgoin, and to examine the evidence upon which his views were based. The French chemist ("Ann. de Chimie," 1868) treats of the electrolysis of sulphuric acid merely as an illustration of his method for determining the composition of hydrated salts in solution generally. This method consists in electrolysing a given solution in a divided cell, analysing the liquid in each compartment at the close of the experiment, and, in the case of dilute sulphuric acid, collecting the hydrogen set free. In the case of a solution of sulphuric acid, of course, the positive compartment may be expected to increase in strength as a consequence of the electrolytic action, and the negative compartment to decrease in strength in the same degree. Bourgoin calls the increase of the acid in the positive compartment x, and concludes that $2x$ represents the amount of sulphuric acid electrolysed. This conclusion rests on the well-known theoretical views of Grotthuss, and, did his theory express all that goes on in the electrolytic process, the method would readily discriminate between the actions represented by the following formulæ:

	Before Electrolysis.		After Electrolysis. Positive pole.	Negative pole.
(1.)	SO_3H_2O	=	SO_3+O	H_2
(2.)	SO_33H_2O	=	SO_3+O_3	H_6
(3.)	SO_3nH_2O	=	SO_3+O_n	H_{2n}

But it was pointed out by Reuss, as far back as 1807, that when electrolytic action occurs across a permeable diaphragm, a portion of the liquid may travel from the positive to the negative compartment of the compound cell by what is now called electrical endosmose. Daniell and Miller ("Phil. Trans.," 1844) pointed out that in electrolytic action there was also an unequal transference of the ions. It is evident that each of these actions must introduce additional terms into the formula with which Bourgoin worked. Moreover, Daniell ("Phil. Trans.," 1839) investigated the electrolysis of sulphuric acid of very different strengths by a similar method, and in a review of the evidence ("Phil. Trans.," 1840) concluded that for each equivalent of hydrogen liberated, the acid which passed across the diaphragm was not more than one-fourth nor less than one-fifth of an equivalent. Most of his experiments incline to the former. Did $2a$, therefore, represent the amount of sulphuric acid electrolysed, it would appear from his results that *tetra-*, and not *hexa-*, basic sulphuric acid was decomposed by the current. Again, Hittorf, in 1853 (*vide* Wiedemann's "Electricity," vol. ii, p. 589), observed that for one equivalent of hydrogen liberated, the amount of sulphuric acid which was found in the positive compartment varied, but not regularly, with the strength of the acid. These discrepancies, both of observation and deduction, led us to make some experiments on the subject ourselves.

The apparatus we employed consisted of a U-shaped tube of about 70 cub. centims. capacity, having a stop-cock in the centre of the horizontal part. The vertical parts of the apparatus were divided into millimetres, and the hole in the stop-cock packed with asbestos.



We found that the closeness of the packing could be so nicely adjusted as scarcely to allow mechanical admixture of the fluids, on the one hand, or electrical endosmose on the other. In our experiments we varied the current density, and, unlike Bourgoin ("Annales de Pharmacie," vol. xv), found that the increase of sulphuric acid in the positive compartment per equivalent of hydrogen set free decreased along with the decrease in the current density. The strength of acid was 4·2 per cent. The results are set out in the annexed table.

Current in milli-ampères.	Time in hours.	Increase of sulphuric acid in positive compartment for one part of hydrogen set free.
32·8	20	9·17
33·4	6	9·5
72·3	2·5	10·3
72·7	2	9·4
106	2	11·0
117	2·5	10·5
215	1·5	12·05
220	1	12·04
229	2	12·31

It is necessary also to bear in mind the remarkable phenomenon called by the Germans "Wanderung der Ionen." Daniell long ago described an experiment in which he placed dilute sulphuric acid in the positive compartment and a solution of sulphate of copper in the negative. He found that when 15·5 grs. of copper had been deposited on the negative electrode there were 23 grs. of sulphuric acid in the same compartment. Now, as 15·5 grs. of copper are equivalent to 24 grs. of sulphuric acid, and as Bourgoin's formula allows for the formation of only half an equivalent of sulphuric acid, that is 12 grs., it is evident that there was a considerable accumulation of that substance unaccounted for. In two similar experiments made with our apparatus, we obtained for 0·147 and 0·125 grm. of deposited copper respectively 0·209 and 0·180 grm. of free sulphuric acid. The half equivalents would be 0·114 and 0·097 grm. respectively. The excess of acid in all these cases is due to the "unequal transference of the ions." If both compartments had been filled with sulphuric acid, some similar transference would doubtless have taken place, in addition to what is expressed in Grotthuss' chain of decomposition.

We conclude, therefore, from our own results, as well as from those of previous experimenters, that the method employed is incapable of determining whether it is H_2SO_4 or some hydrate which yields to the current.

Copper Sulphate.

A careful examination of the chemical changes which accompany the electrolysis of a solution of copper sulphate appeared, however, capable of throwing additional light on the value of this electrolytic method for ascertaining the composition of hydrates in aqueous solution. It is well known that water forms with $CuSO_4$ a definite hydrate, $CuSO_45H_2O$. The anhydrous salt is white, the hydrate blue. It is reasonable to suppose that the blue solution of this salt contains molecules of this or of some higher hydrate. Now, if in the electrolytic process the water of hydration suffers decomposition along with the $CuSO_4$, the primary chemical changes might be expected to be—

$$\text{(A.)}\quad CuSO_45H_2O \;=\; \overbrace{SO_3+O_6}^{\text{Positive pole.}} \quad \overbrace{Cu+5H_2}^{\text{Negative pole.}}.$$

But, if the water of hydration takes no more part in the electrolysis than the water of solution does, then the chemical changes would manifestly be—

$$\text{(B.)}\quad CuSO_4 \;=\; \overbrace{SO_3+O}^{\text{Positive pole.}} \quad \underset{}{\overset{\text{Negative pole.}}{Cu}}.$$

Of course the collateral action—

$$H_2O \;=\; \overset{\text{Positive pole.}}{O} \quad \overset{\text{Negative pole.}}{H_2}$$

might also take place, but this would occur only with currents of considerable density. The method is obviously capable of discriminating between these two actions, even supposing a considerable quantity of the electrolyte travelled unchanged from one compartment of the apparatus to the other. For, in the first case, either free hydrogen would be liberated at the negative pole, or free acid formed in the negative compartment, equal to five-sixths of the total copper deposited; the free acid, and the five-sixths of the total copper, to which it is equivalent, being produced by the chemical action— $5H_2+5CuSO_4=Cu_5+5H_2SO_4$; equation A becoming—

$$CuSO_4,5H_2O+5CuSO_4 \;=\; \overbrace{SO_3+O_6}^{\text{Positive pole.}} \quad \overbrace{Cu_6+5H_2SO_4}^{\text{Negative pole.}}.$$

On the other hand, if the action was in accordance with B there would be only a deposition of copper on the negative electrode, and no formation of free acid in the negative compartment. In the annexed table the results and particulars of several experiments are set out. In each experiment 25 cub. centims. of a 10 per cent.

solution of sulphate of copper were placed in the negative and positive compartments respectively of the apparatus. The positive electrode consisted of a platinum wire, and the negative of a weighed strip of metallic copper.

Experiment.	Time in hours.	Free sulphuric acid.	
		Pos. Compart.	Neg. Compart.
I.	1½	·0766	nil.
II.	2	·0936	nil.
III.	3	·1868	·0191
IV.	3	·1501	·0204
V.	3	·2442	·0237
VI.	3	·2546	·0372

In none of these experiments was there any trace of hydrogen visibly escaping from the negative electrode, while, as will be seen from the table, there was no free acid formed in the negative compartment till two hours or more had elapsed. By that time some admixture in the horizontal part of the apparatus might reasonably be expected, and even in the greatest instance it is small as compared with the amount of salt decomposed.

Similar experiments were made with the sulphate of zinc, with similar results, no hydrogen being evolved, and little or no sulphuric acid appearing in the negative compartment.

We conclude, therefore, that it is not possible to determine the composition, or even to show the presence of a hydrated salt in aqueous solution by means of this electrolytic method.

III. "On the Dynamics of a Rigid Body in Elliptic Space." By R. S. HEATH, B.A., D.Sc., Fellow of Trinity College, Cambridge. Communicated by A. CAYLEY, LL.D., Sadlerian Professor of Pure Mathematics in the University of Cambridge. Received January 4, 1884.

(Abstract.)

This paper is an attempt to work out the theory of the motion of a rigid body under the action of any forces, with the generalised conceptions of distance of the so-called non-Euclidean geometry. Of the three kinds of non-Euclidean space, that known as elliptic space has been chosen, because of the perfect duality and symmetry which exist in this case. The special features of the method employed are

the extensive use of the symmetrical and homogeneous system of co-ordinates given by a quadrantal tetrahedron, and the use of Professor Cayley's co-ordinates, in preference to the "rotors" of Professor Clifford, to represent the position of a line in space.

The first part, §§ 1–21, is introductory; in it the theory of plane and solid geometry is briefly worked out from the basis of Professor Cayley's idea of an absolute quadric. By taking a quadrantal triangle (*i.e.*, a triangle self-conjugate with regard to the absolute conic) as the triangle of reference, the equations to lines, circles, and conics are found in a simple form, and some of their properties investigated.

The geometry of any plane is proved to be the same as that of a sphere of unit radius, so that elliptic space is shown to have a uniform positive curvature.

The theory is then extended to solid geometry, and the most important relations of planes and lines to each other are worked out.

The next part treats of the kinematics of a rigid body. The possibility of the existence of a rigid body is shown to be implied by the constant curvature of elliptic space, and then the theory of its displacement is made to depend entirely on orthogonal transformation. Any displacement may be expressed as a twist about a certain screw. A rotation about a line is shown to be the same as an equal translation along its polar; so that the difference between a rotation and a translation disappears, and the motion of any body is expressed in terms of six symmetrical angular velocities. An angular velocity ω, about a line whose co-ordinates are a, b, c, f, g, h, is found to be capable of resolution into component angular velocities $a\omega, b\omega \ldots h\omega$, about the edges of the fundamental tetrahedron.

The theory of screws is next considered. A twist on a screw can be replaced by a pair of rotations about any two lines which are conjugate to each other in a certain linear complex. The surface corresponding to the cylindroid is found to be of the fourth order with a pair of nodal lines. Lastly, the condition of equivalence of any number of twists about given screws is investigated.

In kinetics, the measure of force is deduced from Newton's second law of motion, and the laws of combination and resolution are proved. The consideration of the whole momentum of a body suggests the idea of moments of inertia, and a few of their properties are investigated. The general equations of motion referred to any moving axes are then found, and in a particular case they reduce to a form corresponding to Euler's equations; these are of the type

$$A\dot{\omega}_1-(B-H)\omega_2\omega_6-(G-C)\omega_5\omega_3=Q_1.$$

The last part is occupied in the solution of these equations when no forces act, in terms of the Theta-functions of two variables. A solution is obtained in the form

$$\omega_1=a\,.\,\frac{\vartheta_0(x\,,y)}{\vartheta_{12}(x\,,y)},\qquad \omega_4=f\,.\,\frac{\vartheta_7(x\,,y)}{\vartheta_{12}(x\,,y)},$$

$$\omega_2=b\,.\,\frac{\vartheta_2(x\,,y)}{\vartheta_{12}(x\,,y)},\qquad \omega_5=g\,.\,\frac{\vartheta_5(x\,,y)}{\vartheta_{12}(x\,,y)},$$

$$\omega_3=c\,.\,\frac{\vartheta_9(x\,,y)}{\vartheta_{12}(x\,,y)},\qquad \omega_6=h\,.\,\frac{\vartheta_{14}(x\,,y)}{\vartheta_{12}(x\,,y)},$$

where $x=nt+\alpha$ and y is arbitrary. But in order that these values may satisfy the equations, a relation among the parameters of the Theta-functions must be satisfied. This is

$$c_6c_{10}c_5c_9+c_1c_{13}c_2c_{14}=0.$$

The solution is not complete, because after satisfying the equations of motion only four constants remain to express the initial conditions, whereas six constants are required.

IV. "Evidence of a Large Extinct Lizard (*Notiosaurus dentatus*, Ow.) from Pleistocene Deposits, New South Wales, Australia." By Professor OWEN, C.B., F.R.S. Received January 9, 1884.

(Abstract.)

In this paper the author describes a fragment of jaw with teeth of a fossil from the pleistocene deposits at the "Cuddie Springs," New South Wales, transmitted by E. S. Wilkinson, Esq., of the Department of Mines, Sydney.

A series of comparisons are detailed with known recent and fossil Saurians, and the microscopic test is applied to the tissues of the bone and tooth.

The conclusion arrived at is that the fossil was part of a lacertian reptile, equal in size to the *Megalania*, but of carnivorous habits; distinct from the largest existing toothed and pleurodont lizard (*Hydrosaurus giganteus.*) For the much larger extinct pleurodont Saurian the author proposes the name *Notiosaurus dentatus.*

January 24, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "Observations on the Influence of certain Culture Fluids and Medicinal Reagents in the Growth and Development of the *Bacillus tuberculosis*." By C. THEODORE WILLIAMS, M.A., M.D., F.R.C.P., Physician to the Hospital for Consumption, Brompton. Communicated by Sir JOSEPH FAYRER, K.C.S.I., F.R.S. Received December 28, 1883.

[Publication deferred.]

II. "The Effects of Lesions of Different Regions of the Cerebral Hemispheres." By DAVID FERRIER, M.D., LL.D., F.R.S., and GERALD F. YEO, M.D., F.R.C.S. Received January 19, 1884.

(Abstract.)

This paper contains a detailed account of a series of observations on the effects of localised lesions of the brain of monkeys, made partly by Drs. Ferrier and Yeo, and partly by Dr. Ferrier alone.

The operations were performed under anæsthetics, and according to the rules and methods of antiseptic surgery.

The experiments are illustrated by photographs, showing the position and extent of the lesions and general condition of the brains, together with sun-prints and microphotographs of sections of the brain and spinal cord, showing the structures primarily involved or secondarily degenerated.

As the paper is mainly an anatomical description of the lesions delineated and record of the effects produced, it scarcely admits of condensation in the form of an abstract. The lesions are classified according to their position in the occipito-angular, temporal, Rolandic, frontal, and hippocampal regions respectively.

Among the more important results arrived at are the following:—

Lesions of the occipito-angular region (occipital lobes and angular gyri) cause affections of vision, without affection of the other sensory faculties or motor powers.

The only lesion which causes complete and permanent loss of vision in both eyes is total destruction of the occipital lobes and angular gyri on both sides.

Complete extirpation of both angular gyri causes for a time total blindness, succeeded by lasting visual defect in both eyes.

Unilateral destruction of the cortex of the angular gyrus causes temporary abolition or impairment of vision in the opposite eye—not of a hemiopic character.

Deep incisions may be made in both occipital lobes at the same time, or the greater portion of one or both occipital lobes at the same time may be removed without any appreciable impairment of vision.

Destruction of the occipital lobe and angular gyrus on one side causes temporary amblyopia of the opposite eye and homonymous hemianopia of both eyes, towards the side opposite the lesion.

As in none of the cases recorded, either of partial unilateral or bilateral destruction of the occipito-angular region, were the amblyopic or hemianopic symptoms permanent, it is concluded that vision is possible with both eyes if only portions of the visual centres remain intact on both sides.

Destruction of the superior temporo-sphenoidal convolution on both sides causes complete and permanent loss of hearing, without other sensory or motor defect. Hearing is not impaired by lesions of any other part of the temporal lobe.

Destructive lesions of the Rolandic zone (convolutions bounding the fissure of Rolando) cause motor paralysis, without loss of sensation, limited (monoplegia) or general (hemiplegia), according to the position and extent of the lesion.

Lesions of this region are followed by descending degeneration of the pyramidal tracts of the spinal cord.

Lesions of the frontal region vary according as they affect the post-frontal (base of the frontal convolutions) or pre-frontal (anterior two-thirds of the frontal convolutions) region.

Lesions of the pre-frontal regions alone do not produce any discoverable physiological symptoms.

Lesions of the post-frontal regions cause temporary paralysis of the lateral movements of the head and eyes.

As the symptoms are only temporary so long as any portions of the frontal lobes remain intact, it is concluded that the post-frontal and pre-frontal regions have essentially the same physiological signification, and that portions only of the frontal lobes are sufficient for the exercise of their functions.

Lesions of the frontal lobes are followed by descending degeneration of the mesially situated tracts of the foot of the crus cerebri and corresponding fibres of the internal capsule, as seen in transverse sections. The ultimate destination of these tracts is uncertain. They cannot be traced in the anterior pyramids.

Lesions entirely destroying the hippocampal region (the hippocampus major and gyrus hippocampi) and neighbouring inferior temporosphenoidal region (without implication of the crus cerebri, basal ganglia, or internal capsule) cause complete anæsthesia—cutaneous, mucous, muscular—of the opposite side of the body, without motor paralysis.

The degree of impairment of tactile sensibility in those cases where it is not entirely abolished, is in proportion to the amount of destruction of the hippocampal region.

Lesions of the cornu ammonis alone, or gyrus hippocampi alone, do not cause permanent impairment of tactile sensibility. The duration of the effects of total destruction of the hippocampal region has not been determined.

No impairment of tactile sensibility has been observed in connexion with any of the other cerebral lesions described.

January 31, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Pursuant to notice Anton De Bary, Carl Gegenbaur, Leopold Kronecker, Rudolph Virchow, and Gustav Wiedemann were severally balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "Determination of the Vertical and Lateral Pressures of Granular Substances." By ISAAC ROBERTS, F.G.S., F.R.A.S. Communicated by J. F. BATEMAN, F.R.S. Received November 6, 1883.

PART I.—*Wheat and Peas.*

The investigations which I have the honour to submit in this communication have been undertaken to furnish data to engineers and others who are concerned with the erection of structures which have to sustain pressure upon floors and retaining walls, and also to further the objects of science in a field that is believed to be new.

Store-houses have been erected, both in this country and in America, which consist of rectangular cells, called *bins*, and are filled with grain to a height of 50 feet and upwards above the ground.

Diligent inquiries have been made for any scientific data or rules by which the necessary strength of the walls and floors of such structures could be computed, or, for instance, what would be the vertical and lateral pressure of a column, say of wheat, 14 feet square at its base and top, and 60 feet in height. It was generally assumed to be something less than the pressure of a similar column of water, but how much less did not appear to be known either in England or America.

Last year I made experiments to obtain data, by employing models of square and hexagonal *bins*, particulars of which were communicated in a paper read in Section G, at the meeting of the British Association for the Advancement of Science, held at Southampton. I give briefly, in tabular form, the results.

Table of the Results of Experiments made to ascertain the Vertical Pressure of Wheat stored in Cells or Bins.

Description of cell.	Length of one side.	Diameter of inscribed circle.	Area of bottom.	Height of cell.	Height of maximum pressure.	Mean pressure on bottom.	Weight of wheat in cell when full.	Number of trials made.
	ins.	ins.	ins.	ins.	ins.	lbs.	lbs.	
Square........	7	7	49	36	11	8·5	46·8	17
Hexagon......	4	7	41·57	60	12·5	7	70·2	26
"	$6\frac{7}{8}$	12	122·8	60	24	48	202·8	18
"	12	$20\frac{3}{4}$	374·11	96	36	224	1014	19

It will be observed that all pressure upon the bottom ceases in each cell, at a point not exceeding in height two diameters of its inscribed circle. In the 7-inch square cell, the pressure ceases at the height of 11 inches. In the 7-inch hexagonal cell, it ceases at the height of $12\frac{1}{2}$ inches. In the 12-inch hexagonal cell, it ceases at the height of 24 inches, and in the $20\frac{3}{4}$-inch hexagonal cell, it ceases at the height of 36 inches. So the eighty trials which were made with the four cells point clearly to one conclusion, namely, *that in any cell which has parallel sides, the pressure of wheat upon the bottom ceases when it is charged up to twice the diameter of its inscribed circle.*

The weight of a cubic foot of wheat when filled loosely is 49 lbs., and the weight when shaken and pressed into the measure is 53 lbs. With these data, and those given in the foregoing Table, we may determine the pressure-figure, or form, which the wheat assumes in the cell. Let us take the 12-inch hexagonal cell as the type. Its area is equal to 122·8 square inches. All pressure upon the bottom ceased when the wheat was 24 inches in height, and the mean pressure upon the bottom then was 48 lbs., which is very nearly the weight of 1 cubic foot of wheat when loosely filled. Then 122·8 square inches × 24 inches = 2947·2 cubic inches, but the pressure was only equal to 1728 cubic inches of wheat (1 cubic foot) and, therefore, the pressure-form must be some figure containing approximately 1728 cubic inches of wheat. We have three dimensions of the figure ascertained by experiment, namely, the area of its base, its height, and its cubical capacity, or, 122·8 ins. the base, 24 ins. the height, and 1728 cubic inches the capacity. The figure that meets these conditions is therefore *parabolic*, and I suggest the following formula to determine its cubical capacity and pressure upon any given area:—

Let a = area of cell in feet.
d = diameter of inscribed circle in feet.
c = the constant determined by experiment.
w = weight of wheat in pounds per cubic foot.
p = pressure upon the bottom of cell in pounds.

If we apply this formula to the cell now under consideration, we have—

$$a \times d \times c \times w = p,$$

or $$\left\{ \frac{122{\cdot}8 \text{ ins.}}{144 \text{ ins.}} \times 1 \times 1{\cdot}03 \times 53 \text{ lbs.} \right\} = 46{\cdot}56 \text{ lbs.}$$

The actual pressure was ascertained by weighing to be 48 lbs., and this was the total mean pressure upon the bottom of the cell, though it was filled with wheat that weighed 202·8 lbs. I now know, and will give the particulars later on, that if this cell had been lengthened to 60 ft. or any other height, the pressure upon the bottom would not have exceeded 48 lbs.

The investigations could not be considered conclusive if left at this stage, since they in a remarkable manner appeared to be contradictory to all our generally received knowledge of the laws governing the flow and pressure of fluids. It was also reasonable to doubt their reliability if applied on a large scale, notwithstanding their close agreement with each other in all the experimental cells. Besides, there was no evidence furnished by the experiments to show what pressure the sides of the cells would have to sustain, although it might be inferred that the side pressure upon a unit of surface, would be less than that upon a similar unit of bottom surface, but how much less, if any, was not indicated. I therefore decided to try the experiments on a large scale, and to include in them determinations both of the vertical and lateral pressures. These I now proceed to describe.

Mr. Paul, the well-known corn-merchant of Liverpool, at the suggestion of Mr. Grayson, architect (both of whom took great interest in the inquiry), placed at my disposal part of a warehouse at the Duke's Dock, together with the use of grain, men, and machinery for manipulating with. In one corner of the warehouse I constructed a rectangular *bin*, two sides of which were formed with spruce deal planks, 3 inches in thickness, placed on end, so that they were supported only at the level of each floor, and had a clear bearing in the ground storey, of 9 ft. 3 ins. The other two sides of the *bin* were the walls of the warehouse. On plan the *bin* measured internally 6 ft. 9 ins. by 6 ft. 0 in., and 52 ft. 2 ins. in height, and it contained when filled 2112 cubic feet of wheat, weighing 111,976 lbs.

The weighing apparatus consisted of two graduated levers, one for weighing the vertical, and the other for weighing the lateral pressures.

They were made according to my directions by the well-known machine makers, Messrs. Henry Pooley and Sons. Each lever was capable of weighing with great accuracy from one pound weight to 1800 lbs., advancing one pound at a time, by a sliding weight, which was caused to run along the graduated edge of the lever by the pulling of a cord. The woodcut on page 236 shows the levers and the other adjuncts placed in position for weighing. The upper figure shows the machine for weighing lateral pressures, and the lower figure that for weighing vertical pressures. Both levers were tested by me with standard weights up to 800 lbs., and half a pound weight would clearly move the lever when any part of the 800 lbs. was applied to the machine. I now proceed to describe the methods of weighing.

The bottom of the *bin* was formed 4 feet above the basement floor of the warehouse, and an opening 3 feet square was made in it centrically. Beneath this opening a slide was placed which could be forced through the grain by means of a powerful screw-jack, or be withdrawn from beneath the grain by means of ropes and pulley-blocks as might be required. Beneath this slide were grooves into which a frame could be inserted, and when so placed it would be concentric with the 3-ft. aperture already described in the bottom of the *bin*. Three frames were made to fit into these grooves, with apertures of 1 ft. by 1 ft., 2 ft. by 2 ft., and 3 ft. by 3 ft. respectively. Beneath any one of the apertures when placed in position in the grooves, was the board which formed the top of the weighing-machine, and the arrangements were such that a column of grain measuring 1 ft., 4 ft., or 9 ft. superficial area, and of any height, up to 51 ft. 9 ins., could at any time be weighed upon the lever of the machine, without the necessity of emptying the *bin* to insert the *aperture* frames after each weighing had been completed. It was also necessary, in order to insure accuracy, that all pressure should be taken off the machine before proceeding with each successive weighing, and these conditions were attained by opening and closing the slides in manner already referred to.

The weighings of the lateral pressures were accomplished in manner slightly different from those of the bottom or vertical. The lever of the machine was placed at right angles with the side of the *bin*, in the way shown on the figure. The pressure-receiving top of the machine, and its bracket, were suspended by a chain from their common centre of gravity, and when placed in their position for weighing, they just touched (without exerting any pressure) the knife-edged fulcra of the lever, and a pressure equal to less than one pound weight would always be indicated by the lever rising off its rest.

Three apertures of the respective areas of 1 ft., 4 ft., and 9 ft.,

were employed in weighing the side pressures, and the slide for admitting and stopping the grain between the weighing-machine and the *bin* was similar to that described for bottom pressure, only that it ran on edge instead of horizontally. The details are shown on the figure.

The method adopted in weighing both the vertical and lateral pressures was the following: On three sides of the *bin*, painted gauge boards were affixed and marked in feet, and each foot was subdivided into spaces of 3 inches. These boards extended from the top of the weighing-machines to the top of the *bin*, and the quantity of grain which I desired to weigh was put into the *bin* and carefully levelled at the desired height, by the aid of these marks. Then three or more weighings were taken at that level. Afterwards the height of the grain in the *bin* was increased to the next desired stage, and then levelled and weighed three times, or more, and so on, stage by stage, until the *bin* was filled.

Before the grain was allowed to come in contact with the weighing-machines, care was taken always to place an excess of weight upon the levers, and this was reduced to indicate the pressure by moving the sliding weight by means of the cord along the graduated edge of the lever. The reduction of the weight on the weighing-machine was therefore steady and gradual.

I had not proceeded far with the process of weighing, when I observed a constantly recurring stiffness in the rising of the levers off their rests. They would rise, say one-fiftieth of an inch off the rest, and there remain for an indefinite length of time without the least further movement; but as 1 lb. weight after another was removed off the lever, it gradually rose until the balance was finally overcome, when the lever would rise and touch the check which prevented further motion upwards. The difference between the first movement and the final rise of the lever frequently equalled one-half the whole pressure. This condition invariably accompanied every weighing. The machines were carefully examined, but no fault lay there, and I infer that the difference here referred to represents the elastic force of grain when it is subject to pressure, and that this elastic force expends itself before the mass of the grain comes into the state of mobility.

In the following tables, which contain the results of each reliable weighing which was made, I distinguish between the first small movement of the lever and the final movement which represents the grain in motion, by the words *Dormant* and *Active* pressures. I call the pressure *dormant* when the lever has been lifted off its rest to the extent that a beam of light can be seen between the lever and its rest; and the pressure *active*, when the lever has risen about three-fourths of an inch above its rest up to the check.

Table I.—Weighings of the Bottom or Vertical Pressures of Wheat.

Size of the Aperture used=3 ft. 0 in. × 3 ft. 0 in.=9 square feet.

	Wheat 6 ft. high in bin.		Wheat 8 ft. high in bin.		Wheat 8 ft. 9 ins. high in bin.		Wheat 51 ft. 9 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	..	938						
	..	924						
	..	928						
	..	874						
	1202	1034	1159	924	1146	1003	1191	826
	1150	1024	..	..		..	1182	828
	1119	957	..	..	..	..	1145	832
Mean	1157	954·14	1159	924	1146	1003	1172·67	828·67

Table II.—Weighings of the Bottom or Vertical Pressures of Wheat.

Size of the Aperture used=2 ft. 0 in. × 2 ft. 0 in.=4 square feet.

	Wheat 3 ft. high in bin.		Wheat 4 ft. high in bin.		Wheat 6 ft. high in bin.		Wheat 51 ft. 9 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	..	..	..	502				
	..	..	..	500	..	..	638	344
	..	457	..	494	..	..	685	370
	..	453	600	500	..	..	702	349
	..	473	585	444	..	494	701	352
	..	461	592·5	488	..	494	681·5	353·75

Table III.—Weighings of the Bottom or Vertical Pressures of Wheat.

Size of the Aperture used=1 ft. 0 in. × 1 ft. 0 in.=1 square foot.

	Wheat 2 ft. 4 ins. high in bin.		Wheat 51 ft. 9 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	..	50	193	50
	..	50	226	50
	..	50	229	50
Mean	..	50	216	50

Table IV.—Weighings of the Side or Lateral Pressures of Wheat.

Size of the Aperture used=3 ft. 0 in. × 3 ft. 0 in.=9 square feet.

	Wheat 6 ft. high in bin.		Wheat 7 ft. 3 ins. high in bin.		Wheat 9 ft. high in bin.		Wheat 45 ft. 6 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	272	225						
	279	265						
	..	214	..	..	..	224	367	218
	335	268	385	300	..	227	350	285
	330	235	300	250	..	220	313	250
	261	202	325	259	..	230	365	240
Mean	295·4	234	336·67	269·67	..	255·25	348·75	248·25

Note.—In all the weighings of side pressures the height of the wheat in the bin is measured from the *lower edge* of the apertures respectively.

Table V.—Weighings of the Side or Lateral Pressures of Wheat.

Size of the Aperture used=2 ft. 0 in. × 2 ft. 0 in.=4 square feet.

Wheat 2 ft. high in bin.		Wheat 4 ft. high in bin.		Wheat 6 ft. high in bin.		Wheat 6 ft. 9 ins. high in bin.		Wheat 8 ft. high in bin.		Wheat 45 ft. high in bin.	
Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
68	43	..	..	..	..	..	..	165	83		
96	38	..	..	..	..	..	..	167	78		
95	40	150	83	160	82	170	80	160	79	238	100
75	39	128	81	150	82	158	87	174	80	206	92
..	..	..	..	..	..	..	..	167	89	195	87
83·5	40	139	82	155	82	164	83·5	166·6	81·8	213	93

Table VI.—Weighings of the Side or Lateral Pressures of Wheat.

Size of the Aperture used=1 ft. 0 in. × 1 ft. 0 in.=1 square foot.

	Wheat 1 ft. high in bin.		Wheat 2 ft. high in bin.		Wheat 4 ft. high in bin.		Wheat 44 ft. 6 ins. in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	..	..	14·5	11	..	..	44	10
	8	2	13	6	15	6	47	6
	11	2	16	6	15	6	41	8
Mean	9·5	2	14·5	7·76	15	6	44	8

Table VII.—Weighings of the Bottom or Vertical Pressures of Peas. Size of the Aperture used=3 ft. 0 in. × 3 ft. 0 in.=9 square feet.

	Peas 9 ft. high in bin.		Peas 49 ft. 6 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	1323 1308	1255 1236	1374 1391	1324 1324
Mean...	1315·5	1245·5	1382·6	1324

Table VIII.—Weighings of the Bottom or Vertical Pressures of Peas. Size of the Aperture used=2 ft. 0 in. × 2 ft. 0 in.=4 square feet.

	Peas 6 ft. high in bin.		Peas 8 ft. 7 ins. high in bin.		Peas 49 ft. 10 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	723 690	544 597	738 746	601 561	646 726	506 644
Mean...	706·5	570·5	742	581	686	575

Table IX.—Weighings of the Side or Lateral Pressures of Peas. Size of the Aperture used=3 ft. 0 in. × 3 ft. 0 in.=9 square feet.

	Peas 6 ft. high in bin.		Peas 9 ft. high in bin.		Peas 43 ft. 10 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	193 185	116 119	227 251	167 161	242 239	201 187
Mean...	189	117·5	239	164	240·5	194

Table X.—Weighings of the Side or Lateral Pressures of Peas.

Size of the Aperture used=2 ft. 0 in. × 2 ft. 0 in.=4 square feet.

	Peas 6 ft. high in bin.		Peas 43 ft. 9 ins. high in bin.	
	Dormant pressure in lbs.	Active pressure in lbs.	Dormant pressure in lbs.	Active pressure in lbs.
	97 90	67 53	121 118	89 89
Mean...	93·5	60	119·5	89

In the experiments made with the model *bins*, which have been already referred to, the bottom of the model was formed by a loose board, which rested upon an ordinary weighing-machine, and it only just touched the lower edges of the boards forming the sides. The weights were removed a pound at a time off the machine, and the pressure of the wheat was indicated by the bottom falling away, and the wheat consequently running out; but in the large *bin* the bottom measured 6 ft. 9 ins. by 6 ft. 0 in., and therefore it was not practicable to weigh the whole of the pressure upon the bottom in the manner adopted with the models. I therefore decided to weigh columns of grain within the bin, having a base of 9 ft., 4 ft., and 1 square foot respectively. These columns were formed of the mass of the grain contained within the *bin*, and being coincident with its centre, were not subject to any friction against the sides, but only to the friction amongst the particles of the grain.

Referring now to Table I, it deals with a column of wheat having a base area of 9 square feet. The mean *dormant* pressure when 6 ft. in height was 1157 lbs.; when 8 ft. in height it was 1159 lbs.; when 8 ft. 9 ins. in height it was 1146 lbs.; and when 51 ft. 9 ins. in height it was only 1172·67 lbs. One of the weighings at 6 ft. in height showed the pressure to be 1202 lbs.; but the greatest pressure indicated with the height or head of 51 ft. 9 ins. was only 1191 lbs.

The *active* pressure is in every case less at the height of 51 ft. 9 ins. than it is at any height below that. At 6 ft. it reached 1034 lbs; but at 51 ft. 9 ins. it did not exceed 832 lbs.

The actual weight of the column of wheat that indicated these pressures was, in the case of 6 ft. in height, 2862 lbs. (=6ft. 0 in. × 9 ft. 0 in. × 53 lbs.); and in the case of 51 ft. 9 ins. in height, 24,685 lbs. =(9 ft. 0 in. × 51 ft. 9 ins. × 53 lbs.).

In Table II the column of wheat had a bottom area of 4 square feet, and with the height of 51 ft. 9 ins. would weigh 10,971 lbs.= (4 ft. 0 in. × 51 ft. 9 ins. × 53 lbs.); but the mean *dormant* pressure upon the bottom was only 681·5 lbs., whilst at only 4 ft. in height the mean *dormant* pressure was 592·5 lbs.

The *active* pressure, as will be seen in this table also, is less when the column of wheat is 51 ft. 9 ins. in height than it is at any lower level.

In Table III the aperture had an area of 1 square foot, and the column of wheat 51 ft. 9 ins. in height, indicated a mean *dormant* pressure of 216 lbs., and in each weighing an *active* pressure of only 50 lbs., even at the height of 2 ft. 4 ins., this amount of active pressure was indicated. The total weight of a column of wheat 1 foot square and 51 ft. 9 ins. in height would be 2742 lbs., but the maximum *dormant* pressure did not exceed 229 lbs., and the *active* pressure was only 50 lbs. If now we formulate the results obtained and recorded in Tables I, II, and III in terms of those obtained by the model *bins* already referred to, we have:—

a=area of column of wheat in square feet.

d=diameter of inscribed circle in feet.

c=constant determined by experiment.

w=weight of wheat in pounds per cubic foot.

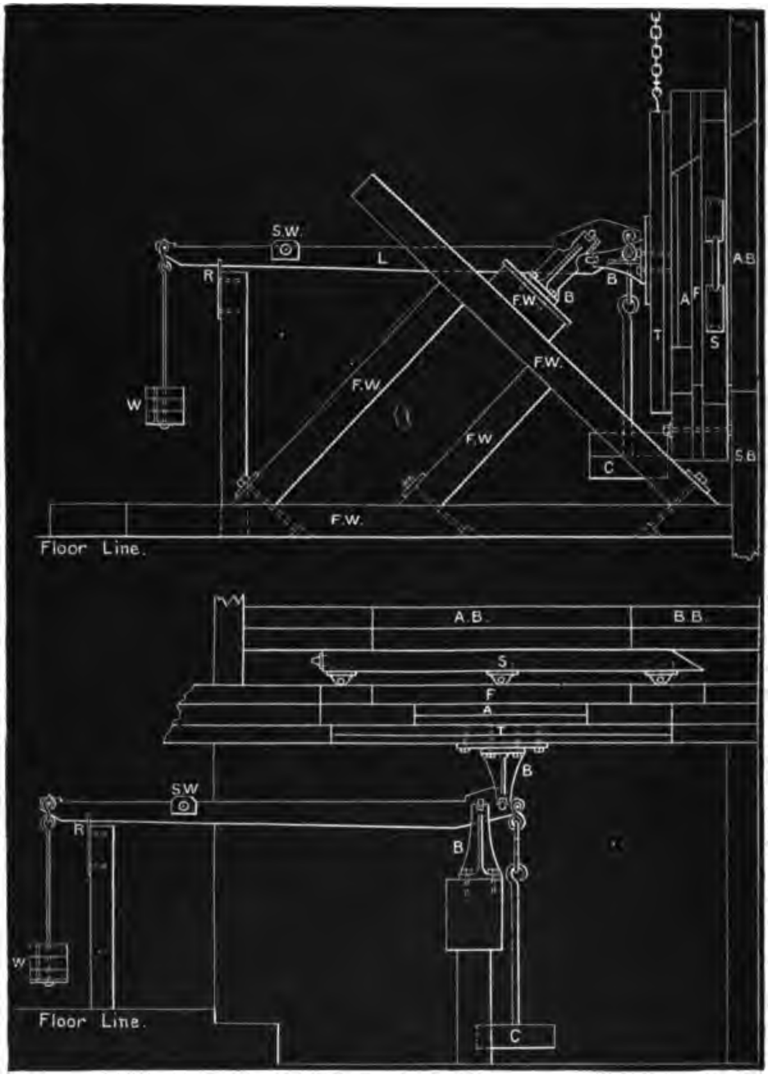
p=pressure upon the bottom.

c is found to be variable within certain limits. With the aperture of 9 square feet, according to Table I, it is 0·84. With the aperture of 4 square feet, according to Table II, it is 1·66, and with the aperture of 1 square foot, according to Table III, it is 4·32.

It, therefore, appears that the apex of the paraboloid which represents the pressure upon 1 square foot area at the centre of a *bin* full of wheat, is higher relatively with its inscribed circle, than it is relatively with the inscribed circle which has its diameter equal to the bottom of the *bin*.

We shall now proceed to consider the lateral or side pressures, and by referring to Tables IV, V, and VI, it will be seen that in each of the weighings, irrespective of the height of the wheat in the *bin*, there is a *dormant* and an *active* pressure both considerably less than the corresponding vertical pressure. With the aperture of 9 square feet the maximum *dormant* pressure of 385 lbs. was attained at the height of 7 ft. 3 ins. above the lower edge of the aperture, and at the height of 45 ft. 6 ins. the maximum pressure was 367 lbs. With the aperture of 4 square feet the maximum *dormant* pressure was 238 lbs., at the height of 45 ft. and at the height of 6 ft. 9 ins. the pressure reached 170 lbs. With the aperture of 1 square foot the maximum *dormant* pressure at 44 ft. 6 ins. in height was 47 lbs.

With the aperture of 2 ft. by 2 ft., and the wheat only 2 ft. in height, that is to say, level with the top edge of the aperture, the *dormant*



pressure was 96 lbs., and when a man whose weight was 170 lbs. stood upon the wheat, close to the aperture, the pressure was 127 lbs., being an increase of 31 lbs. only beyond the pressure of the wheat without the man.

It therefore appears by the weighings of the lateral pressures given in the Tables, that in no case is the maximum *dormant* pressure in a *bin* measuring 6 ft. 9 ins. by 6 ft. on plan, and 45 ft. 6 ins. in

height, greater than 60 lbs. on the square foot of side surface, and that this pressure is nearly uniform between the bottom and 6 ft. off the top, about which point it diminishes gradually to the top.

That the lateral or side pressure cannot be represented, like the bottom pressure, by a parabolic figure, the respective weighings prove, for with the aperture of 1 square foot, and a head of wheat 44 ft. 6 ins. in height, the maximum *dormant* pressure was 47 lbs. With the aperture of 4 square feet, the maximum *dormant* pressure with a head of 45 feet was 59·5 lbs. on each square foot. With the aperture of 9 square feet and a head of 45 ft. 6 ins. the maximum *dormant* pressure was 40·8 lbs. on each square foot.

With the data given in the foregoing pages, we are now in a position to compute both the vertical and lateral pressures, and to determine the distribution of the pressures of the wheat contained in a *bin* of the dimensions given. They are as follows:—

Length of *bin*, 6 ft. 9 ins.; breadth of the *bin*, 6 ft.; height of the wheat, 51 ft. 9 ins.; weight of the wheat, per cubic foot, 53 lbs.; weight of the wheat contained in the *bin*, 111,080 lbs.

Bottom pressure computed by the formula, $a \times d \times c \times w = p$, or the bottom area in square feet into the diameter of the inscribed circle in feet, into the constant, into the weight of wheat in pounds per cubic foot, equal the pressure upon the bottom in pounds. If we substitute the dimensions we shall have (6 ft. 0 in. × 6 ft. 9 ins.) = 40 ft. 6 in. × 6 ft. 5 ins. × 0·84 × 53 lbs. = 11,569 lbs., the pressure upon the bottom.

The side pressure will be the superficial area in feet of the inside of the walls of the *bin* multiplied by 43 lbs., or, 25 ft. 6 ins. × 51 ft. 9 ins. × 43 lbs. = 56,744 lbs., the pressure on the four sides. I adopt 43 lbs., the constant of pressure as shown by the weighings with the 9 square feet aperture, in this calculation, for the reason that it more closely approximates the size of the *bin* than the smaller apertures do. The difference between the sum of the vertical and lateral pressures and the whole of the weight of the wheat contained in the *bin* would be 42,767 lbs., and this would represent the friction of the wheat against the sides. This frictional pressure added to the lateral pressure would represent the weight sustained by the side walls. The wheat is formed into an elastic *plug*, fitting closely to the shape of the *bin*, and each grain is held in sensitive equilibrium by the resultants of the vertical, lateral, and frictional resistances.

If a slide valve were opened in the bottom or side of the *bin* when it is full of wheat, the equilibrium of the grains would be disturbed to the extent of a vertical column having a sectional area somewhat larger than the aperture of the valve, and its height equal to the head of wheat. It would be a vortex column fed mostly by the wheat at the top of the *bin* sliding into it down slopes formed by the angle of

repose (about 30°), and then passing through the mass of the wheat towards the outlet.

The flow of the wheat through an aperture, say of 80 square inches area, can easily be stopped by a board held in the hand against it, though the head may be 50 feet or more.

These statements are not made by inference, for I have proved their accuracy on several occasions. On one I placed some $1\frac{1}{2}$-inch cubes of wood, and a pound weight on the vortex column in a bin containing wheat 37 ft. in height, and they passed through the body of wheat and out at the bottom when 20 ft. of wheat still remained in the *bin*. On another occasion, I am informed by a credible eye-witness, that a cat passed through the mass of wheat by the vortex column and came out alive and uninjured, through the aperture at the bottom of the *bin*. The body of wheat through which it passed exceeded 20 ft. in height.

There is yet one question concerning the lateral pressure to be considered, and that is:—If 43 lbs. equal the amount upon each square foot uniformly from bottom to top in a *bin* which measures 6 ft. 0 in. × 6 ft. 9 ins. × 45 ft. 6 ins., would it be exceeded in a *bin* which measures, say 12 ft. 0 in. × 12 ft. 0 in. × 60 ft. 0 in.? In answering the question, it has already been shown that all lateral pressure ceases at a height not exceeding *two and a-half times* the breadth of the side (see Table IV), and therefore all lateral pressure would cease in the *bin* assumed at the height of 30 ft. (12 ft. 0 in. × $2\frac{1}{2}$); and in order to determine if the lateral pressure would be increased by any increased breadth of the *bin*, I caused one to be constructed within that already described, of the dimensions 3 ft. 0 in. × 3 ft. 0 in. × 12 ft. 0 in., and when it was filled with wheat the maximum *dormant* pressure was 48·55 lbs. upon a square foot. This pressure was also indicated when only 7 ft. of wheat was placed in the *bin*.

This shows that within certain limits the lateral pressure is slightly greater in the small *bin* than it is in the large one; and we may, I think, in practice safely adopt 50 lbs. upon each square foot of side as the constant for the lateral pressure of wheat.

We shall now consider the pressure of peas.

Those selected for the experiment were dry, hard, and round. They weighed, when pressed into the measure, 54 lbs. per cubic foot, and the determinations of the vertical and lateral pressures were made precisely in the same manner, and with the same weighing-machines and *bin* that were used in determining the pressures of wheat. I need not, therefore, repeat any part of the descriptions which have been already given, and will now discuss the tables which embody the results obtained. They are numbered respectively VII, VIII, IX, and X.

Table VII shows the maximum *dormant* pressure upon the bottom

aperture of 9 square feet with the head or height of 49 ft. 6 ins. of peas to be 1391 lbs., and with the head of 9 ft. the pressure of 1323 lbs. With the bottom aperture of 4 square feet (Table VIII) and the head of 49 ft. 10 ins., the maximum *dormant* pressure of 726 lbs. was indicated, and with the head or height of 6 ft., 723 lbs. The apex of the pressure paraboloid is therefore at the height of three diameters of the inscribed circle of the base or aperture, and is greater by one diameter than the wheat paraboloid. The constant (c) for peas is, in the case of the 9 ft. aperture, 0·96; the corresponding constant for wheat being 0·84.

The lateral pressures are given in Tables IX and X, where, with the aperture of 9 square feet and a head of 43 ft. 10 ins. of peas, the maximum *dormant* pressure was 242 lbs., and with the head of 9 ft. 251 lbs. With the aperture of 4 square feet and head of 43 ft. 9 ins. the maximum *dormant* pressure was 121 lbs., and with a head of 6 ft. 97 lbs. It therefore appears that all lateral pressure ceases at a height equal to three times the breadth of the aperture as indicated by the 9 ft., and that the pressure is slightly greater as indicated by the 4 ft. square aperture.

With the 9 ft. it is 27·89 lbs. on each square foot, and with the 4 ft. it is 30·25 lbs. The pressures of wheat corresponding with these (Tables IV and V) were 42·78 lbs. and 59·5 respectively.

The conclusions to be drawn are, that the vertical or bottom pressure of peas is *greater* than that of wheat in the ratio of 0·96 to 0·84, or of 1·73 to 1·66; but that the lateral or side pressure of peas is *less* than that of wheat in the ratio of 27·89 to 42·78, or of 30·25 to 59·5.

The constant (c=0·96) for computing the vertical pressure is safe for the equilibrium of peas, wheat, or any other grain, but the safe permanent sustaining powers of any floors or walls should be at least four times the constant.

For the lateral pressure, 50 lbs. upon each square foot of retaining wall is safe for the equilibrium of all grain; but the safe permanent resistance to pressure in any wall or partition should not be less than four times that, or 200 lbs. upon each foot.

It will have been observed that the *maximum dormant* pressure has been adopted in the formulæ for both the vertical and lateral pressures, though the *active* pressure is that which would cause the breaking down of the structure, and, as shown in the tables, it is considerably less than the *dormant*.

Seeing that the constants for the vertical pressures of both wheat and peas are so near unity, one being 0·84 and the other 0·96, I propose to adopt unity in the formula, which will then stand finally thus:—

$$a \times d \times w = p,$$

or the area of the bottom of the *bin* in feet multiplied by the diameter of its inscribed circle in feet, and the product multiplied by 54 lbs. (the weight of grain per cubic foot), equal the pressure upon the bottom of the *bin* in pounds, whatever height the grain in the *bin* may be, exceeding two diameters of its inscribed circle.

Following are the weights per cubic foot of various kinds of grain, as determined by myself. The loose and close fillings of the measure are given in each case, but the close or pressed filling is adopted as the element W in the formula.

Weight of grain per cubic feet.	Loosely filled into measure.	Closely filled into measure.
Wheat :—	lbs.	lbs.
Red winter	49	53½
Bombay	49	53
California	49	53
Walla Walla	46	50½
Bessarabia	49	53
Peas :—		
American	50	54
Indian corn :—		
White American	43½	47
Mixed ,,	44	47
Oats :—		
Russian	28	33
Beans :—		
Egyptian	46	50
Barley (English)	39	44

EXPLANATION OF THE FIGURE.

A. Aperture through which the grain is brought into contact with the top of the weighing machine.
B. Brackets to support the levers.
A.B. Apertures in the side and bottom of the *bin*.
B.B. Floor of the *bin*.
S.B. Side of *bin*.
F. Free space between *bin* and apertures.
S. Slide for admitting and shutting off the grain.
T. Top of weighing machine.
R. Rest for lever.
S.W. Sliding weight.
W. Suspended weights.
L. Levers.
C. Counterpoise weights.
F.W. Frame of machine.

II. "Notes on the Microscopic Structure of some Rocks from the Andes of Ecuador, collected by E. Whymper, F.R.G.S. No. I. Pichincha." By Professor T. G. BONNEY, D.Sc., F.R.S. Received January 25, 1884.

During his adventurous journey in the Andes of Ecuador in December, 1879, and the earlier months of 1880, Mr. E. Whymper succeeded in ascending several of the highest mountains, and collected a considerable series of rock specimens, which he has entrusted to me for examination. Although descriptions of rocks from this district have already been published by some eminent petrologists on the Continent, yet, as Mr. Whymper's collections were made with great care, and specimens were taken in numerous instances from localities never before touched by the hand of man, I have thought it better to make my notes as far as possible complete, and so have occasionally included specimens of which some account may have appeared in foreign scientific periodicals.

Probably no collection has hitherto been described which either has been so extensive or has consisted of specimens with the localities more exactly determined.

In making these notes I think it better to group the specimens geographically rather than mineralogically, so as to give successively a synoptic view of the products of each volcano, and I select as the subject of the first of a short series of papers the rocks from Pichincha, although from its proximity to Quito, and its accessibility, it is one of the best known of the Ecuadorian Andes, because the collection from this mountain is a comparatively large one, and contains a considerable number of well-preserved specimens.

Pichincha, according to Mr. Whymper, is a dormant and nearly extinct volcano, lying to the north-west of Quito, which city is built almost on the extremities of its eastern slopes. To quote his words, "From the south, north, and east (I have not seen the mountain from the west) Pichincha has the appearance of a range rather than a cone. It rises into several well-marked, but not bold, peaks, the principal of which are Guagua-Pichincha, the highest point, which by my barometrical-mercury observations is 15,918 feet (15,706 Reiss and Stübel Δ), and Rucu-Pichincha, which is only a few hundred feet lower.

"The summits of the mountains Pichincha, Atacatzo, and Corazon lie almost in a direct line, and Illiniza* is only slightly out of it.

* Atacatzo, 14,892 feet (Reiss and Stübel, merc. bar.). Corazon, 15,871 feet (Whymper, merc. bar.). Illiniza, 17,405 feet (Reiss and Stübel, Δ). Quito (Central Plaza), 9,363 feet (Whymper, merc. bar., mean of many observations); Quito (uncertain in what part of city); 9,350 feet (Reiss and Stübel, merc. bar., mean of many observations).

The two northern mountains in themselves assume the appearance of small ranges. The two southern are peaked, but have not the cone-like aspect of Cotopaxi or Sangai (the only really active volcanoes of Ecuador). Each mountain is separated by well-marked depression, the lowest points of which in each case lie about midway between the summits. Thus, though the four mountains may be considered part of a range, they are all well marked off one from another, and are independent; the two northern being sufficiently large to assume for themselves the aspect of small ranges. The base of Pichincha, for instance, from north to south extends over well-nigh 20 miles.

"Pichincha is said (at Quito) to have several craters. I did not *see* any, though I was possibly in one. We were two days camped close to the top of the mountain, but were in a fog for almost the whole time, so that we had no little difficulty in making sure that we were on the true summit, and wholly failed to see the crater or craters, though perhaps we clambered down the sides of one for about 1500 feet. I believe that every person who has ever been up Pichincha, or anywhere near the summit, has been much troubled by want of clearness in the atmosphere. There appeared to me to be strong grounds for believing that there were two principal craters on the highest parts of the mountain, but some persons would have it that there were more than two. Whatever the number of craters, they must be tolerably central, as I have seen all the slopes of the mountain on the south and east of the two summits mentioned above, and there are no craters upon them.

"If I had not had experience with other mountains, which rendered me cautious, from knowing how easily a cloud may mislead an observer, I should say that I have seen steam rising from behind the summit of Guagua-Pichincha while I have been viewing it at a distance. There was, however, a considerable amount of evidence from natives living around in different localities that Pichincha is seen to 'smoke,' and various persons of greater or less credibility have affirmed that they have been to the bottom of the craters, and have seen 'fissures, smoke, and fire.'

"Snow-beds of considerable size remain upon Pichincha permanently; but these lie in deep fissures and ravines, and are not noticed at a distance; hence I should scarcely think it proper to term it a snow mountain."

The specimens from Pichincha which I have examined are twenty-eight in number. Of these, eighteen were obtained by Mr. Whymper from a collector at Quito, and were presented to myself, the remainder were collected by him during his ascent of the mountain. The localities are affixed to the former set of specimens, but as some of the names are unknown to Mr. Whymper, the spots whence these specimens have been taken cannot in every case be precisely determined. Hence I

shall describe the two collections separately. It may be remarked that the labels indicate that in the opinion of the native collectors there is undoubtedly a crater on Guagua-Pichincha.

Of these eighteen specimens, all but one would, without hesitation, be described in general terms as "Trachytes," and that, though rather a dark grey in colour, could hardly receive any other name. The others are either light grey inclining to a yellowish tint, or shades of a pale red inclining to purple.

Four of these specimens bear in common the designation *Cuspide del Rucu-Pichincha*, August 24, 1879. Rucu-Pichincha, as has been said, is the lower or middle summit, whose height, according to Reiss and Stübel, is 15,542 feet. Mr. Whymper informs me that the term *Cuspide* in Ecuador is applied equally to the *bonâ fide* summit of a mountain, or to the general culminating mass. These four specimens have a compact-looking matrix of a pale-reddish colour, in which are scattered numerous little crystals of a whitish felspar. All evidently are rather decomposed. The first bears the additional label *Tioloma chiquito paso de la Guibrada* "*Rumi-huaico.*" The matrix is a warm pinkish-grey in colour, containing numerous felspar crystals, seldom exceeding 0·125 inch in longer diameter, and often less, together with a few specks of a dark mineral. To the earlier stage of consolidation belong the following minerals: (1) Felspar, commonly in crystals about 0·04 inch to 0·02 inch in longer diameter, frequently containing numerous comparatively large, rounded and slightly lobed inclosures of a brown glass, and an occasional speck of ferrite. Oligoclase, sometimes showing zonal banding, is present, but the large extinction angles of many of the crystals indicate that labradorite or anorthite, probably the former, is also abundant. (2) A pyroxenic mineral in well defined crystals; some of these exhibit the usual outline, cleavage, and characteristics of normal augite, but several in their form, pleochroism, and mode of extinction agree with the orthorhombic mineral, lately recognised as hypersthene.* The former are frequently stained of a dark brown colour towards the exterior. (3) Some ill-defined ferruginous spots may also be seen, which sometimes appear to replace a crystal of augite, hornblende (in one instance), or mica.

To the later stage of consolidation belong fairly numerous microliths of a plagioclastic felspar in a grey granulated base. This, by remaining dark in all positions between the crossed Nicols, is proved to be a glass, though somewhat modified by decomposition.

The rock then is an augite-andesite with some hypersthene, and it

* Messrs. Hague and Iddings, "Amer. Journ. Science," vol. xxvi, p. 222. Mr. J. J. H. Teall, "Geol. Mag.," Dec. ii, vol. x, p. 145, &c. Mr. Teall identifies the mineral in the Cheviot porphyrites, and gives references to papers by Mr. W. Cross and others.

bears, under the microscope, a general resemblance to that from La Guardette, Cantal, figured by MM. Fouqué and Lévy, *Roches Éruptives*, Pl. xxix.

Two other specimens, labelled respectively "*Puerta de Tanguieta Encillada para Nina-urcu*" and "*Machai Grande, subida al fichaco de Puguampa, lado norte*," exhibit only varietal differences, so that it is needless to enter into the details of their microscopic structure. The fourth specimen, labelled *Tioloma grande paso a vintunillas*, is a rock of similar character to the other, but it incloses an oval mass of darker colour and more granulated structure, which has a fairly sharp boundary, so that it somewhat resembles an inclosed pebble. The latter is composed of a clear mineral, generally polygonal (approximately hexagonal) in outline, containing some minute microlithic inclosures. This mineral gives occasional indications of zonal banding and oscillatory twinning; if a felspar it is difficult to assign the species; there is also a fair amount of a pale augite (which has subsequently consolidated), and of an iron peroxide in irregular granules, and as a staining in what appears to be the sparse remnant of a glassy base, which gives a faint indication of a fluidal structure. I am disposed to regard the mass as a node rather than an included fragment, caught up by the melted lava.

The next group of nine specimens all bear the label *Crater of Guagua-Pichincha*, the more precise localities being (I translate the Spanish terms) (*a*) *Ascent of the Crater, N.W. slopes*; (*b*) *Idem*; (*c*) *Idem*; (*d*) *Edge of Crater, N.W. side*; (*e*) *Idem, N.E. side*; (*f*) *Commencement of the Descent of the Crater, S.E. side*; (*g*) *Slopes of the Crater, N.E. side*; (*h*) *Bottom of the Crater, near the ancient fumaroles* (a very decomposed specimen); (*i*) *Bottom of the Crater, brink of the inferior Crater*. These specimens all bear a general resemblance to one another, and, as most of them are not very well preserved, I have had only two of them cut for microscopic examination, as I felt certain that the differences which the others might present would be only varietal, and that it was doubtful whether some would be in a fit state for examination. These are the specimens marked (*a*) the best preserved of the series, and (*e*) that from the summit rim of the crater, which may be regarded as about in an average state of preservation.

The former of these specimens (*a*) has a compact pale-grey matrix, in which are scattered little crystals of whitish felspar and rather elongated prisms of a mineral resembling a black hornblende, which do not exceed about 0·2 inch in length, and are often less.

In the first stage of consolidation are: (1) Crystals of felspar, with regular outlines, probably in most instances labradorite; they generally contain, though in variable quantities, microlithic inclosures. These sometimes have a rough zonally banded arrange-

ment, sometimes are collected in the inner part of crystals which have a clear external band. They are mostly a brownish glass, in which gas cavities are rather frequent; others are belonites of a pale colour; (2) hornblende in fairly regular crystals, with well defined cleavage, mostly of a rather dark greenish-brown colour, but occasionally paler; (3) granules, rather rounded in outline, of an iron oxide. In the second stage of consolidation are microliths of a plagioclastic felspar in a glassy base stained with opacite.

The second specimen presents only varietal differences from the above, but is less well preserved. We may accordingly name this series hornblende-andesite.

The next two specimens are labelled *Sistema de Pichincha* (massif of Pichincha), and are distinguished from the other by having a dull-grey matrix, in which crystals of whitish felspar, not exceeding 0·2 inch (and generally less), are scattered. The better suited for examination has the additional indication *Penoneo de Pamescucho, abajo de Nina-urcu.** In microscopic structure this rock is not unlike the first described: it has a glassy base, rather darkened with ferrite and opacite, and crowded with minute crystallites of felspar, in which are scattered crystals of a plagioclase, probably labradorite, with augite and hypersthene. It is, therefore, like the rock from Rucu-Pichincha, a hypersthenifcrous augite-andesite.

The other specimen from *Pichacho de los Ladrillos* has a general resemblance to the last, except that it is more scoriaceous and decomposed. I have not thought it needful to have a section made. It is probably an augite-andesite, and very possibly contains a little hypersthene. The last two specimens are both from Nina-Urcu. They resemble one another and each of the other groups in some respects. The matrix is compact, is a dull reddish colour, and in it are scattered crystals of a whitish felspar, as in the last group, and of a pyroxenic mineral, somewhat as in the second, the general colour coming nearer to that of the first group.

The better preserved one, which I have examined microscopically, is labelled *Cerro de Candela, Encillada entre Rucu y Guagua-Pichincha,* the other *Encillada al Rucu y Guagua-Pichincha.* In the former are seen, in the first stage of consolidation, (1) crystals of a plagioclastic felspar, most, if not all, of which, from its extinction angles, is probably labradorite; (2) crystals of normal augite; (3) crystals of hypersthene, as above described. Cavities and what appear to be glass inclosures are rather numerous in both the felspar and the augite. The ground-mass appears to be a dark-brown glass, all but opaque; but as the slide has offered difficulties to the workman, and is

* Pamascucho, below Nina-urcu. The latter name is applied by Reiss and Stübel to a point in the ridge connecting Guagua-Pichincha with Rucu-Pichincha, the elevation of which they give as 14,472 feet.

thicker than the rest, it is not in a very good state for examination, and crystallites may be more numerous than appear.

The collection made by Mr. Whymper himself consists of a series of specimens taken during his ascent of the highest peak or cone, Guagua-Pichincha, including a specimen broken off the actual summit, and one specimen broken from the summit of Rucu-Pichincha, the middle or second peak of the mountain, by one of his guides, Louis Carrel. The specimens from the slopes of Guagua-Pichincha form one series with the summit rock and with the second group, described above, from the same part of the *massif*. Some, as might be expected, are rather decomposed,* others are vesicular—pumiceous or slaggy varieties of the rock of which those already described are the more compact form. There are minor differences in the quantity of felspar and of the black pyroxenic mineral scattered through the matrix, but these appear to me merely varietal; hence I have only had a slice cut from the specimen which was broken from the topmost crag of Guagua-Pichincha, for this, though generally resembling the specimens described above, exhibits some slight differences. The external surfaces of the fragment have a slightly scoriaceous aspect, and where the lichen-growth is chipped away, are of a dull grey to rusty brown colour. The fractured faces show the matrix to be of a dull, but not dark, colour, in places slightly vesicular, the walls of the hollows being coated with a pellicle of iron-rust. In the matrix are scattered pretty thickly whitish felspar crystals, not generally exceeding 0·2 inch in diameter, and granules or crystals of a black mineral, less than 0·125 inch in diameter, the former being rather larger, and the latter rather smaller than the corresponding minerals in the group of specimens already described from Guagua-Pichincha. Under the microscope we find that (1) felspar crystals of very variable size are scattered throughout the slide, the majority exhibiting polysynthetic twinning. The boundaries generally are sharply rectilinear, but not a few of the largest have evidently been broken, and one or two show signs of external corrosion. Microlithic inclosures are frequent, but very variable in amount. Some of the crystals contain a considerable number of irregular bits of brown glass, with or without granules of pyroxene; others cavities, in which are bubbles, apparently fixed, occupying about one-eighth of the whole volume; others aggregates of exceedingly minute granules, like the finest dust, possibly augite, of which mineral, if I mistake not, belonites occur. As a rule, the smaller crystals are clearer than the larger, and the inclosures are more numerous in the inner than in the outer part, but there is so much variation observable that no law can be established. It is difficult to settle the precise species of the felspar, but the general

* Mr. Whymper in all his collections endeavoured to bring away a fair representation of what he saw on the mountain, rather than a series of choice specimens.

results of my observations are in favour of the majority being labradorite.

(2.) Hornblende. This mineral occurs in crystals, sometimes rather more than 0·05 inch diameter. It is usually of a strong olive-green or brownish-green colour, showing a very marked dichroism, often not conspicuously cleaved, but occasionally exhibiting in cross-sections a very characteristic cleavage. Not seldom the crystals have their outlines rounded, as if worn or somewhat corroded, and are black-bordered. Microlithic inclosures of felspar and of opacite are rather frequent. (3.) A much paler and less dichroic mineral occurring rarely, not black bordered. In one case a crystal of it appears to grow on to the broken face of a hornblende crystal. It contains microliths of opacite but not of felspar. The extinction angles indicate that this also is hornblende, but I consider it to be formed subsequently to the dark variety. (4.) Two or three clusters of a granular mineral of a pale colour, which I believe to be augite. (5.) One or two crystals which, from their dichroism and extinction, I consider hypersthene. (6.) Granules of iron oxide, probably magnetite. The interstitial matter is of a dusty grey-brown colour, and, when examined with high powers, appears to consist of a transparent glass crowded with minute earthy particles and specks of ferrite. The rock then which forms the topmost crag of Guagua-Pichincha is, as our previous investigation would lead us to expect, a hornblende-andesite, though not quite so normal a specimen as some of those already examined.

The specimen broken from the highest point of Rucu-Pichincha is a compact grey rock, in very fair preservation, containing scattered crystals of a glassy felspar up to about 0·2 inch in diameter, and smaller specks of a black pyroxenic mineral. Microscopic examination shows the felspars to be similar to those described in the last specimen, except that cavities with bubbles are almost wholly wanting. Other inclosures are frequent, especially nearly colourless belonites. The pyroxenic mineral is rather abundant: some is light brown, the rest of a greenish tint. Some of the former is certainly augite, some of the latter hypersthene; probably the minerals may be distinguished by their colours, but this of course cannot be fully proved. Grains of magnetite occur as above. The matrix is often darkened by the presence of specks of kaolin and ferrite, but differs from the last in being crowded with very minute lath-like crystallites of felspar. Probably, however, a colourless base of true glass is present. The rock then is a hyperstheniferous augite-andesite, and thus agrees with the specimens already described from this peak.

The annexed analysis* is that of a rock, apparently from some distance up the peak of Guagua-Pichincha. From the description, how-

* Roth. Die Gesteins-analysen. 35. Analysis quoted from Abich. Vulk. Ersch. 1841. 58.

ever, it would seem to be a blacker and compacter rock than any that I have seen, more like some of those from Antisana.

"Guaguapichincha aus 14,238 Fuss Höhe." Black, pitchstone-like, small albite, green augite, magnetite (glassy andesite).

SiO_2	67·07	Sp. gr. 2·579
Al_2O_3	13·19	
Fe_2O_3	4·74	
Mn_2O_3	0·32	
CaO	3·69	
MgO	3·46	
K_2O	2·18	
Na_2O	4·90	
Ignition	0·30	
TiO_2	trace	
	99·85	

From the above descriptions, it may be, I think, fairly inferred that the main peak or cone of Pichincha consists chiefly of hornblende-andesites, and the second summit (Rucu-Pichincha) of hypersthen-iferous augite-andesites; and that there is considerable uniformity in the character of the rocks of this volcanic mountain.* Those from Antisana, which it is intended to describe in the next communication, are of a rather more varied nature.

III. "Report on the Tidal Disturbances caused by the Volcanic Eruptions at Java, August 27 and 28, 1883, and the Propagations of the 'Supertidal' Waves." By Major A. W. BAIRD, R.E. Communicated by Lieutenant-General J. T. WALKER, R.E., F.R.S., with a letter from the Communicator containing an Abstract of the contents of the Paper.

Received January 24, 1884.

(Abstract.)

At the request of Major A. W. Baird, R.E., I herewith forward for presentation to the Royal Society, a report which he has prepared on the Tidal Disturbances caused by the now famous volcanic eruptions which occurred on the 27th and 28th August last year, in the Island of Krakatoa and the Straits of Sunda, between Sumatra and Java. For

* I have seen, in collections of rocks from Hungary, specimens bearing considerable resemblance to those from Pichincha: for instance, an "amphibol-andesite" from Altsohl is like those of Guagua-Pichincha, and one from Blanfuss, Kremnitz, like specimens from Nina-urcu and Rucu-Pichincha.

some years past it has been Major Baird's special duty, as an officer of the Great Trigonometrical Survey of India, to supervise the operations for the investigation of tidal phenomena which have been carried on under the orders of the Government at a number of points on the Indian coast lines from Kurrachee round to Moulmein, and also at Aden. A tidal observatory, containing a self-registering tide-gauge, and all the necessary meteorological instruments, has been established at each of these points; every observatory has a man in charge who tends the instruments, sets the diagrams to true time as received telegraphically from Madras, and submits daily reports to Major Baird.

After the occurrence of the eruptions at Krakatoa, Major Baird made a careful examination of the diagrams at the whole of his tidal stations, seventeen in number, and at twelve of them he found more or less distinct evidence of tidal disturbance. In the accompanying report he sets forth this evidence, and gives such further facts as he has been able to collect from information supplied by eye-witnesses of the disturbances on the coasts of Ceylon and at other localities where as yet tidal stations have not been established. He also gives the magnitudes of the "supertidal" waves and the intervals between them; shows by calculation the probable time of the great eruption at Krakatoa; and then deduces the velocities of the waves which reached each station. He furnishes copies, on a much reduced scale, of the original diagrams of the tidal registrations of the 27th and 28th August which were made at each of his own stations; and he also gives a chart of a portion of the Indian Ocean which he has prepared to illustrate the course of the first great supertidal wave.

The testimony of the diagrams of self-registering tide-gauges must ordinarily be expected to be much more precise and reliable than that of eye-witnesses. This may be more particularly claimed in the present instance; for the clocks of the gauges were carefully supervised, and their times checked daily, at every station but Port Blair, by Madras time signals, and the diagrams are on so large a scale, 60 inches in length by 24 in breadth, that any sensible variation of sea-level is measurable on them; for it is depicted on the full natural scale wherever the tide range does not exceed 5 feet, and on half that scale where the range exceeds 5 but does not exceed 10 feet, the corresponding time scale being in all cases 1 inch to an hour; and as the stations at which the range of tide is less than 10 feet happen to be those at which the disturbances were greatest, very exact measures of the times and amplitudes of all the supertidal waves, whether small or great, are forthcoming for all the more important stations. The diagrams accompanying Major Baird's report were intended for illustration rather than for measurement; thus they have all been drawn on a much smaller scale than any of the originals,

viz., the uniform scale of 1 inch=3 feet for height and=6 hours for time. The original diagrams are too bulky to be reproduced to full scale for publication; but Major Baird presents in his report all the details which they furnish by giving the numerical elements of the time and height of every appreciable supertidal wave, remarking, with characteristic modesty, that thus the whole of the facts will be available for the further treatment of the subject by more competent hands.

The principal facts set forth by Major Baird are the following:—

1st. The primary effect of the great eruption at Krakatoa was a marked fall in the sea level—or in other words, the formation of a negative supertidal wave—at each of his stations.

2ndly. This negative wave was succeeded by a great positive wave at an interval ranging from seventy-five minutes at Negapatam, the station nearest Krakatoa, to twenty-four minutes at Aden, the most distant station.

3rd. Supertidal wavelets of greater or less magnitude were registered at the whole of the Indian stations some hours, more or less, before the effects of the great eruption; they are evidences of antecedent minor eruptions, the occurrence of which they would establish even in the absence of all other information on the subject. The intervals between these warnings and the great eruption which they heralded range from about three hours at Aden, the most distant station, to eighteen hours at Negapatam, the nearest station. This shows that the explosions were at first comparatively faint and feeble, being felt only at the nearest stations, but afterwards they increased in intensity, becoming sensible even at the most distant station three hours before the effects of the great eruption.

4th. Great supertidal waves of amplitudes ranging from a maximum of 22 inches at Negapatam to a maximum of 9 inches at Aden, were registered at all the stations which were in a position to receive the full force of the eruptions at Krakatoa, unobstructed by the configuration of the foreshore. Other waves of less magnitude, ranging down to half the maxima values, occurred at these stations at intervals of one to two hours for about twelve hours after the first great wave.

5th. The secondary great waves were succeeded by wavelets gradually diminishing in size, but continuing for some time; they are traceable on the diagrams for the 29th and 30th August, the second and third days after the great eruption; they cease first at Port Blair about 8 P.M. of the 29th, at Negapatam at 4 A.M. of the 30th, and, lastly, at Aden at 5 P.M. of the 30th.

6th. Loud reports resembling the firing of distant guns were heard at Port Blair and on the Nicobar Islands on the 26th and 27th August, and being supposed to be signals from a vessel in distress a steamer was sent out in search of the vessel; similar reports were

heard at two places on the coast of Ceylon on the 26th, first at 6 P.M. and afterwards at midnight.*

These facts show clearly that the terrible eruption at Krakatoa, which carried desolation over the surrounding regions and was attended with such an appalling loss of life, was preceded for some hours by minor eruptions which were insignificant only by comparison, for they produced effects which were sensible even at Aden, a distance of upwards of 4000 miles. It is possible, therefore, that some at least of the subsequent supertidal waves may be due to eruptions occurring subsequently to the great eruption.

It is a singular fact that we are still without any precise and definite information of the time at which the great eruption took place. In a very interesting and suggestive note on the Barometrical Disturbances which passed over Europe between the 27th and 31st August—communicated to the Royal Society on the 12th December—General Strachey gives an investigation of the speed of barometric waves travelling from Krakatoa round the earth, some from east to west, others from west to east, and he estimates that the great disturbance which caused the initial barometric rise probably occurred at 9 h. 24 m. local time on the morning of the 27th August. Now Major Baird shows that the primary effect of the great eruption was to produce a recession or fall of the sea-level at each of his tidal stations, and this was also noticed at the Mauritius. It seems probable that the initial barometric rise occurred at the same time as the initial oceanic fall or great negative wave, which is shown to have preceded the first great positive wave by an interval ranging from 75 minutes at the nearest Indian station to "about a quarter of an hour" at the Mauritius. Major Baird has ascertained from Her Majesty's Consul at Batavia that the first great positive wave reached Batavia at 12 h. 10 m. mean local time on the afternoon of the 27th August; he infers from the table of the velocities of free tide waves in Sir George Airy's article on Tides and Waves in the "Encyclopedia Metropolitana," that as the distance from Krakatoa to Batavia is about 105 miles, and the average depth of the sea between them 186 feet, the wave must have taken about two hours to travel that distance. Thus, allowing five minutes for difference of longitude, the local time of the occurrence of the first great positive wave at Krakatoa would be 10 h. 5 m. A.M., or about 1 h. 40 m. later than General Strachey's estimated time of the initial barometric rise. If, however, we assume this time to have been identical with that of the initial oceanic fall, and to have preceded the first great positive wave by an interval somewhat greater at

* The times throughout Major Baird's report are referred to the meridian of Port Blair, in lat. 11° 41′ N., and long. 92° 45′ E. of Greenwich, the easternmost tidal station at which the tides were disturbed.

Krakatoa than at the nearest Indian station—an inference to be drawn from the fact that all the recorded intervals between the negative and positive waves increase as the distances from the centre of disturbance diminish—the difference between General Strachey and Major Baird will practically disappear. When we consider the absolute independence of the two methods of deduction which they have respectively employed, the facts of the one being derived from the atmosphere, while those of the other are derived from the ocean, the coincidence between the two results appears very striking.

The Admiralty chart of the Eastern Archipelago shows that Krakatoa is situated at the focus of what may be regarded as a parabolic figure formed by the configuration of the contiguous portions of the coasts of Java and Sumatra; the axis of the figure is directed westwards towards the Indian Ocean, and nearly at the apex there is an opening to the north-east formed by the Straits of Sunda. Thus the waves generated by an eruption at Krakatoa would be mostly propelled towards the Indian Ocean, both directly and, though in a minor degree, by reflection from the coasts; the rush of waters towards the coasts would only have one outlet, namely, the Straits of Sunda, through which, as is well known, a great wave passed, carrying widespread destruction for some distance beyond, along the contiguous coasts of Java and Sumatra. This wave may possibly have impinged with great force on the south-west coast of the island of Borneo, which is on the prolongation of a straight line drawn from Krakatoa through the Straits of Sunda. But to the north-west, at Singapore, no trace of tidal disturbance appears to have been met with; a self-registering tide-gauge is established there, and the distance from Krakatoa is less than one-third of that of the nearest Indian station; but the Master Attendant reports that the gauge records "no difference whatever in the tide." This may be due to the fact that the wave through the Straits of Sunda, when it bent round to the north-west towards Singapore, had but a shallow sea to advance over, and its course must have been greatly impeded by the numerous islands and shoals and the narrow straits and passages between them; thus it may have well broken up and disappeared at some distance short of Singapore.

In an appendix to his report, Major Baird gives the respective velocities with which the great primary positive wave travelled to Galle, the Mauritius, the coast of Africa, and three of his own stations. He necessarily assumes that the as yet unknown time at which this wave issued from Krakatoa is to be fixed from the known time of its advent at Batavia, by deducting two hours from the latter in accordance with the velocity table in Airy's Tides and Waves already referred to. He obtains for the maximum velocity 467 statute miles per hour, both for Port Louis, in the Mauritius, distant

3400 miles, and for Port Elizabeth, in South Africa, distant 5450 miles; that the same value should be obtained for two places, one of which is 2050 miles further from the origin than the other, shows that there is not likely to be any gross error in the time adopted for the starting of the wave from Krakatoa; the value is, moreover, interesting in that it is practically identical with Airy's velocity of a free tide-wave over an ocean 15,000 feet deep, which is approximately the depth of the ocean on this line; in other directions the velocities are less, namely, towards Galle, 397; Negapatam, 355; and Aden 371 miles. In the calculations the velocity has been assumed uniform in each instance, as a matter of convenience, but it must in reality have been greatest wherever the ocean depth was greatest. The greatest depth is known to occur on the lines to Ports Louis and Elizabeth, for which we have the greatest velocities; but the wave which impinged on the Indian stations, and eventually reached Aden must for a considerable portion of its course have been identical with the wave which impinged on Ports Louis and Elizabeth, and then it must have travelled with the same high velocity; afterwards, on passing over shallower oceans, its velocity must have been materially retarded, and may have fallen much below the averages, 355 to 397 miles, for the whole course. This appears probable, moreover, from the evidence of the earthquake in the Bay of Bengal on the 31st December, 1881, of which an account—communicated by myself—was published in the "Proceedings of the Asiatic Society of Bengal" for March, 1883. The position of the centre of impulse has been fairly well fixed at a point nearly on the line between Port Blair and Negapatam, and the time is known within a few minutes. The supertidal waves caused by this earthquake at the contiguous tidal stations much exceeded in magnitude the waves caused at the same places by the eruption at Krakatoa, but their velocity was found to range between 240 and 120 miles an hour, varying with the general depth of the water traversed and the configuration of the foreshore at the respective stations. It is probable, therefore, that for the latter portion of its course the wave from Krakatoa to the same stations travelled with no greater velocity than on the occasion of the earthquake in the Bay of Bengal.

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"On a New Standard of Illumination and the Measurement of Light." By William Henry Preece, F.R.S. Received and read June 21, 1883.

1. The present position of photometry as practically applied to electric lighting is by no means in a satisfactory condition. Mathematical accuracy in the measurement of the intensity of the light emitted by a given source is unobtainable owing to the numerous disturbing influences of quality and quantity which render impossible any graduated scale of light. An *absolute* standard of light has not yet been obtained. I have long felt that to meet the case of electric

light illumination we must not depend upon any direct comparison between the light emitted by the source to be measured and any given recognised standard of light; but that we should rather make our standard of comparison an area illuminated to a given intensity whatever be the source of light. We do not want to know so much the intensity of the light emitted by a lamp, as the intensity of illumination of the surface of the book we are reading, or of the paper on which we are writing, or of the walls upon which we hang our pictures, or of the surface of the streets and of the pavements upon which the busy traffic of cities circulates. Illumination of this character depends not on one source only, but on many sources of light distributed in innumerable ways. Hence, I propose to measure the illumination of surfaces quite independent of the sources of light by which they are illuminated. The difficulties in comparing the illumination of spaces have hitherto been insurmountable, but now, thanks to the beautiful little glow lamps introduced by Mr. Swan, these difficulties have disappeared.

The standard I propose is the space illuminated by 1 British standard candle at 12·7 inches distance, which is the same as the illumination given by 1 French standard "bec" on the same space at 1 metre distance.

2. The principle of measurement which guides us in nearly all the methods at present in practical use is to compare two surfaces equally illuminated to the same intensity by lights placed at different distances, and to compare them by the law of inverse squares determined by Kepler. This law, however, assumes a clear space through which the light is transmitted, and the emission of this light from a point. Distance therefore becomes essential, and the error introduced by large flames or by the diffusion of light becomes difficult to eliminate; indeed, by the present system it is well-nigh impossible to measure satisfactorily the light-giving power of several lamps distributed over a room or large area. Moreover, the principle ignores the presence of different colours, and it involves the necessity, with electric arc lamps, of taking measurements at different angles.

Up to the present time, the simplest and perhaps the most favourite mode of measurement is Rumford's plan of contrasting shadows, but Bouguer's method of comparing equally illuminated surfaces, as carried out by Ritchie and Bunsen, is also in great favour.

3. All these plans, however, are dependent upon the establishment of a uniform standard of light. Although in England, Parliament has legalised the standard candle, it has been found so inconvenient in practice, owing to its want of uniformity, that Mr. Vernon Harcourt has felt it advisable to replace the standard spermaceti candle with a flame resulting from the combustion of pentane. In France, again, the standard is the Carcel bec, burning at the rate of 0·0042 kilo. of

pure colza oil per hour, with a flame of 40 millims.; while Draper (1847), Zöllner (1859), and Schwendler (1879) proposed a given length or surface of platinum raised to incandescence by a current of electricity as a useful standard. At the Congress held in Paris last year it was suggested that a convenient standard would be the light emitted by a square centimetre of incandescent platinum maintained at its fusing point, but up to the present moment no one seems to have devised what really may be called an absolute standard of white light. The question is now being considered by a Committee of the British Association. If it were possible to find a convenient, simple, fixed, and uniform material which, having a resistance of 1 ohm, could be raised to incandescence by 1 ampère flowing through it, we should have a convenient standard, but this has not yet been obtained.

As I have said, the standard of comparison I propose is a uniformly illuminated surface. I thought at one time that we might have made use of sunlight for this purpose, but I find that sunlight and moonlight are even more variable than the light produced by artificial means, and, after innumerable trials, I have come to the conclusion that the standard which I have proposed is one very easily obtained, and though not absolutely fixed, can be made as nearly uniform and reproducible as any standard hitherto suggested. It is true that it is of a secondary character, and dependent primarily upon a recognised standard source of light; but this primary standard remains in the laboratory, while the measurement can be made in the streets, on board ship, or wherever the surface of illumination may be.

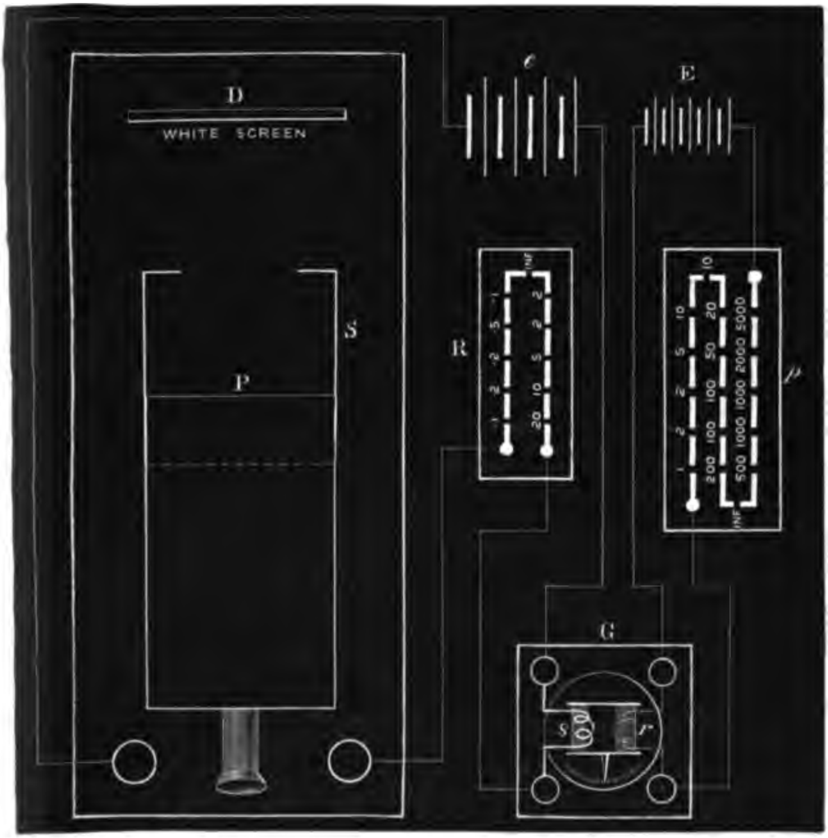
4. Having thus fixed upon a standard of illumination it became desirable to devise an instrument that would enable us to compare areas differently illuminated.

FIG. 1.

For this purpose I make use of a small Swan glow lamp giving $2\frac{1}{2}$ candle power with the current produced by 5 volts. The construction of the photometer is shown by figs. 1 and 2. The lamp (L) is fixed

inside a cylinder or box (B) blackened in the interior, over the end of which is stretched a diaphragm of drawing paper (P); the latter has a round grease spot in the centre about the size of a shilling, as in Bunsen's photometer. At about 12 inches distant from the end of the tube a screen (D) of white drawing paper is fixed. The dia-

FIG. 2.



phragm (P) is so screened by a sliding cover that no light falls upon it but that which is reflected from the drawing paper (D). At the end of the tube where the glow lamp L is set is an eyepiece, through which the illuminated surface of the diaphragm at the end of the tube can be observed. Beneath the base of the instrument is fixed a secondary battery (*e*, fig. 2) which enables us to transmit a current of any desired strength through the glow lamp (L). A rheostat (R) is fixed in circuit with this battery, and enables the current to be

adjusted at will; the strength of the latter is read on a graduated electrodynamometer, or upon a galvanometer.

For laboratory purposes a more accurate mode of measuring the intensity of the current is the following:*—

G is a galvanometer wound with two coils, one (*s*) consisting of a short length of thick wire, and the other (*r*) of a long length of thin wire. In circuit with the latter coil is a resistance, *p*, and a standard battery E.

The relative deflective values of the coils *s* and *r* for similar currents being as 1 to *n*, then when balance is obtained on the galvanometer, the current C flowing through the coil *s* will be—

$$C=\frac{nE}{r+p},$$

r being the resistance of the galvanometer coil and battery, and E the E.M.F. of the battery E.

Thus if the relative deflective values of the coils *s* and *r* are as 1 to 473, that is, if $n=473$, then if E consist, say, of a 5-cell Daniell battery, and the resistance (*r*) of the galvanometer coil and battery be 800 ohms, the current C, when balance is obtained by adjustment of *p*, will be—

$$C=\frac{473\times 5\times 1\cdot 07}{800+p}=\frac{2530}{800+p}.$$

Practically with this arrangement a change of $\frac{1}{1000}$th of an ampère in the current through the coil *s* can easily be detected, and currents can be read to three places of decimals.

To work the instrument it is only necessary to place the screen (D) in the place whose illumination is to be determined, and then by means of the rheostat (R) to lower or increase the current passing through the lamp until the grease spot, when viewed through the eyepiece, becomes invisible, then the light reflected on the diaphragm and transmitted through the grease spot by the distant surface of drawing paper will be equal to the light thrown upon the opposite side of the diaphragm by the glow lamp. The current producing the light thus becomes a measure of the illumination of the screen.

The following measurements were made to determine the current that gave various degrees of illumination—the source of light being a standard candle.

* Cardew, "Proceedings of Telegraph-Engineers and Electricians," vol. ii, p. 301.

Distance of source of light from illuminated surface.	Equivalent degree of illumination.	Current through lamp required to produce balance of illumination. (C.)	$C^6 \times 15 \cdot 994$.
Feet.		Ampères.	
·50	64·000	1·260	64·000
·75	28·445	1·100	28·335
1·00	16·000	·959	12·442
2·00	4·000	·790	3·888
3·00	1·778	·690	1·726
4·00	1·000	·651	1·217

From these experiments it appears that the illuminating power of the glow lamp increases in the ratio of the sixth power of the current. Hence it is only necessary to read the current that gives the necessary equivalent to determine the degree of illumination.

The current in ampères thus gives, for the particular glow lamp employed, a number whose sixth power expresses the amount of the illumination measured.

The apparatus works with ease and regularity, but it has this difficulty—that it is dependent upon the constancy of the lamp, a point not yet absolutely attained. The glass envelope of the lamp becomes deadened by use; the carbon fibre of the lamp gradually becomes deteriorated; and the vacuum sometimes fails. These operations, however, are very slow ones, and it is only necessary occasionally to compare either the light given by unit current with that in the laboratory thrown upon a surface by a standard candle or by one of Mr. Vernon Harcourt's pentane lamps.

It is, however, certain from numerous experiments and practical trials that the light produced by the incandescence of a given carbon filament due to the passage of a given current is more easily reproduced from one time to another, and is more uniform than probably any other artificial standard of light.

February 7, 1884.

THE PRESIDENT in the Chair.

Dr. David Gill was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Motion of Fluid, part of which is moving Rotationally and part Irrotationally." By M. J. M. Hill, M.A., Professor of Mathematics at the Mason Science College, Birmingham. Communicated by Professor G. G. STOKES. Sec. R.S. Received December 29, 1883.

(Abstract.)

Clebsch has shown that the components of the velocity of a fluid u, v, w, parallel to rectangular axes x, y, z, may always be expressed thus—

$$u=\frac{d\chi}{dx}+\lambda\frac{d\psi}{dx},\ v=\frac{d\chi}{dy}+\lambda\frac{d\psi}{dy},\ w=\frac{d\chi}{dz}+\lambda\frac{d\psi}{dz};$$

where λ, ψ are systems of surfaces whose intersections determine the vortex lines; and the pressure satisfies an equation which is* equivalent to the following—

$$\frac{p}{\rho}+\mathrm{V}=-\frac{d\chi}{dt}-\frac{1}{2}\left\{\left(\frac{d\chi}{dx}\right)^2+\left(\frac{d\chi}{dy}\right)^2+\left(\frac{d\chi}{dz}\right)^2\right\}+\frac{1}{2}\lambda^2\left\{\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2+\left(\frac{d\psi}{dz}\right)^2\right\}$$

where p is the pressure, ρ the density, and V the potential of the forces acting on the liquid.

It is shown in this paper that an equation in λ only can be obtained in the following cases (that is to say, as in cases of irrotational motion, the determination of the motion depends on the solution of a single equation only) :—

(1.) Plane motion, referred to rectangular co-ordinates x, y.

λ being a function of x, y, t, let $\frac{d\lambda}{dy}$ be expressed as a function of λ, x, t by substituting in it for y its value in terms of λ, x, t found

* British Association Report for 1881, p. 62.

from the equation which gives λ as a function of x, y, t. Let the result be denoted by $\left(\frac{d\lambda}{dy}\right)_{t}^{\lambda}{}_{x}$.

Let differentials of λ, x, t, regarded as independent variables, be $\delta\lambda$, δx, δt respectively.

Thus $\int\frac{\delta x}{\left(\frac{d\lambda}{dy}\right)_{t}^{\lambda}{}_{x}}$ is an integral taken with regard to x, λ and t being considered constant.

$\frac{\delta}{\delta t}\int\frac{\delta x}{\left(\frac{d\lambda}{dy}\right)_{t}^{\lambda}{}_{x}}$ is the differential coefficient of the above integral with regard to t, λ and x being considered constant.

$\int\delta x\left\{\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dy}}\right\}_{t}^{\lambda}{}_{x}$ denotes that $\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dy}}$ is expressed as a function of λ, x, t, and the result integrated with regard to x, λ and t being considered constant.

F and H are the symbols of arbitrary functions.

Then the equation in λ is shown to be—

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}\right)\left\{\frac{\delta F(\lambda,t)}{\delta t}+\int\delta x\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dy}}\right)_{t}^{\lambda}{}_{x}\right\}=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{\delta x}{\left(\frac{d\lambda}{dy}\right)_{t}^{\lambda}{}_{x}}\right\}\right]$$

and the current function is

$$\frac{\delta F(\lambda,t)}{\delta t}+\int\delta x\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dy}}\right)_{t}^{\lambda}{}_{x}$$

For a vortex of invariable form moving parallel to the axis of y with velocity $\frac{dY}{dt}$ (where Y is a function of t only) the equation in λ becomes—

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}\right)\frac{\delta F(\lambda,t)}{\delta t}=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{\delta x}{\left(\frac{d\lambda}{dy}\right)_{t}^{\lambda}{}_{x}}\right\}\right],$$

and the current function is

$$\frac{\delta F(\lambda,t)}{\delta t}-x\frac{dY}{dt}.$$

(2.) Plane motion, referred to polar co-ordinates r, θ.

With notation similar to that in the preceding case, the equation in λ is—

$$\left(\frac{d^2}{dr^2}+\frac{1}{r}\frac{d}{dr}+\frac{1}{r^2}\frac{d^2}{d\theta^2}\right)\left\{\frac{\delta F(\lambda,t)}{\delta t}+\int r\delta r\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{d\theta}}\right)_{t}^{\lambda}\right\}$$

$$=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{r\delta r}{\left(\frac{d\lambda}{d\theta}\right)_{t}^{\lambda}}\right\}\right].$$

The current function is

$$\frac{\delta F(\lambda,t)}{\delta t}+\int r\delta r\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{d\theta}}\right)_{t}^{\lambda}.$$

For a vortex of invariable form rotating about the origin with angular velocity $\frac{d\omega}{dt}$, this equation becomes

$$\left(\frac{d^2}{dr^2}+\frac{1}{r}\frac{d}{dr}+\frac{1}{r^2}\frac{d^2}{d\theta^2}\right)\left(\frac{\delta F(\lambda,t)}{\delta t}-\frac{r^2}{2}\frac{d\omega}{dt}\right)=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{r\delta r}{\left(\frac{d\lambda}{d\theta}\right)^{\lambda}}\right\}\right],$$

and the current function is

$$\frac{\delta F(\lambda,t)}{\delta t}-\frac{r^2}{2}\frac{d\omega}{dt}.$$

(3.) Motion symmetrical with regard to the axis of z in planes passing through it, referred to cylindric co-ordinates r, z.

The equation in λ is—

$$\frac{1}{r^2}\left(\frac{d^2}{dr^2}-\frac{1}{r}\frac{d}{dr}+\frac{d^2}{dz^2}\right)\left\{\frac{\delta F(\lambda,t)}{\delta t}+\int r\delta r\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dz}}\right)_{t}^{\lambda}\right\}$$

$$=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{r\delta r}{\left(\frac{d\lambda}{dz}\right)_{t}^{\lambda}}\right\}\right].$$

The current function is

$$\frac{\delta F(\lambda,t)}{\delta t}+\int r\delta r\left(\frac{\frac{d\lambda}{dt}}{\frac{d\lambda}{dz}}\right)_{t}^{\lambda}.$$

For a vortex of invariable form which moves parallel to the axis of z with velocity $\frac{dZ}{dt}$, this equation becomes—

$$\frac{1}{r^2}\left(\frac{d^2}{dr^2}-\frac{1}{r}\frac{d}{dr}+\frac{d^2}{dz^2}\right)\frac{\delta F(\lambda,t)}{\delta t}=H\left[\lambda,\left\{\frac{\delta F(\lambda,t)}{\delta\lambda}-\int\frac{r\delta r}{\left(\frac{d\lambda}{dz}\right)_r^{\lambda}}\right\}\right],$$

and the current function is

$$\frac{\delta F(\lambda,t)}{\delta t}-\frac{r^2}{2}\frac{dZ}{dt}.$$

Suppose that in any of these cases any particular integral of the equation in λ is taken.

It is shown that the components of the velocity can be expressed in terms of λ and differential coefficients of λ, and that the current function is also known.

In the case of a fluid, part of which is moving rotationally and part irrotationally, the boundary surface separating the rotationally moving fluid from that which is moving irrotationally contains the same vortex lines, and may be taken as the surface $\lambda=0$.

Now, if the integral taken of the equation in λ do actually correspond to a case of fluid motion in which part of the fluid is moving rotationally and part irrotationally, the most obvious way to find the irrotational motion will be to find its current function from the conditions supplied by the fact that the components of the velocity are continuous at the surface $\lambda=0$. If after taking any integral of the equation in λ it be found theoretically impossible to determine the current function of an irrotational motion outside the surface $\lambda=0$, which shall be continuous with the rotational motion inside it, then the integral in question does not correspond to such a case of fluid motion.

In this method no assumption is made as to the distribution of the vortex lines (as in the method of Helmholtz) before commencing the determination of the irrotational motion.

On the other hand, it is not a particular case of a method applicable to motion in space of three dimensions.

But it can be shown that Clebsch's forms for the components of the velocity do also lead to a method which is applicable to the determination of the irrotational motion when the rotational motion in space of three dimensions is known.

For the rotational motion being known, the components of the velocity are known for this part of the fluid. Let the components of the velocity be expressed in Clebsch's forms, so that χ, λ, ψ are known.

Moreover, let the forms be so arranged that the surface separating

the rotationally moving fluid from that which is moving irrotationally is the surface $\lambda=0$.

Then at this surface the components of the velocity are $\frac{d\chi}{dx}, \frac{d\chi}{dy}, \frac{d\chi}{dz}$.

Now, obtain in any manner a velocity potential ϕ for space outside $\lambda=0$ continuous with χ all over the surface $\lambda=0$. This is theoretically possible always.

If the velocity potential so obtained make the velocity and pressure continuous all over the surface $\lambda=0$, then a possible case of motion will have been obtained.

The conditions to be satisfied in order that the velocity may be continuous at the surface $\lambda=0$ are that there $\frac{d\chi}{dx}=\frac{d\phi}{dx}, \frac{d\chi}{dy}=\frac{d\phi}{dy}, \frac{d\chi}{dz}=\frac{d\phi}{dz}$. In order that the pressure may be also continuous, it is further necessary that $\frac{d\chi}{dt}=\frac{d\phi}{dt}$ all over the surface $\lambda=0$.

The most obvious way of obtaining the velocity potential will be to apply Helmholtz's method of finding the components of the velocity in terms of the supposed distribution of magnetic matter throughout the space occupied by the rotationally moving fluid.

It must, however, be remembered, as is remarked by Mr. Hicks in his report to the British Association on "Recent Progress in Hydrodynamics," Part I,* "That the results refer to the cyclic motion of the fluid as determined by the supposed distribution of magnetic matter, and do not give the most general motion possible." It appears also from Examples I and III of this paper that it is not possible to assume arbitrarily the distribution of vortex lines, even when it can be shown that the equations of motion are satisfied at all points where the fluid is moving rotationally, and then to proceed to calculate the irrotational motion by means of the supposed distribution of magnetic matter. For in these examples, values of the components of the velocity of a rotational motion, satisfying the equations of motion throughout a finite portion of the plane of x, y, are found. Thus the distribution of vortex lines, and, therefore, that of the supposed magnetic matter over a finite portion of the plane of x, y is known. The surfaces that always contain the same vortex filaments are found. Inside one of these the supposed magnetic matter is distributed, the current function at external points is calculated by Helmholtz's method, and it is shown that the velocity so found is not continuous with the velocity at the surface, which separates the rotationally moving liquid from that moving irrotationally.

Another way (suggested by Clebsch's forms) of obtaining the velocity potential will be as follows:—

* Report for 1881, Part I, p. 64.

Calculate the quantity $\rho = -\frac{1}{4\pi}\left(\frac{d^2\chi}{dx^2}+\frac{d^2\chi}{dy^2}+\frac{d^2\chi}{dz^2}\right)$.

Treating ρ as the density of a material distribution inside $\lambda=0$, taking no account of the value of ρ outside the surface $\lambda=0$, obtain the potential of this distribution. Let the potential inside $\lambda=0$ be χ', and outside let it be ϕ.

χ' will, in general, differ from χ; first, because χ may contain many-valued terms, which may be denoted by P satisfying Laplace's equation; and, secondly, because $\chi - P$ may be the potential of a distribution of matter, part of which is outside $\lambda=0$.

Accordingly, it is necessary to examine whether it is possible to find many-valued terms P satisfying Laplace's equation such that $\chi'+P=\chi$.

Then $\phi+P$ will be the velocity potential of the irrotational motion, provided that it give zero velocity at infinity.

The few illustrations which follow are a first attempt to apply the above theory to particular cases.

Example 1. x, y being rectangular co-ordinates in a plane,

Y any function of the time t, $\dot{Y}$ its differential coefficient with regard to the time.

f, a, b, constants,

u, v the components of the velocity parallel to the axes,

it is shown that $u=(f)\frac{2(y-Y)}{b^2}$; $v=\dot{Y}-(f)\frac{2x}{a^2}$ satisfy the equations of motion.

The surfaces containing the same particles are the elliptic cylinders $(f)\left(\frac{x^2}{a^2}+\frac{(y-Y)^2}{b^2}\right)=\text{constant}$.

For a finite portion of the plane of x, y outside the cylinder $\frac{x^2}{a^2}+\frac{(y-Y)^2}{b^2}=1$, the current function of an irrotational motion continuous with that inside the cylinder is—

$$(f)\,\frac{a^2+b^2}{ab}\log\left(\sqrt{a^2+\epsilon}+\sqrt{b^2+\epsilon}\right)$$

$$+\frac{2f}{a^2-b^2}\left\{\frac{2}{a^2-b^2}\left(\epsilon+\frac{a^2+b^2}{2}\right)\left(v+\frac{a^2+b^2}{2}\right)+\frac{a^2-b^2}{2}\right\}$$

$$-\frac{2(f)(a^2+b^2)}{ab(a^2-b^2)^2}\sqrt{(a^2+\epsilon)(b^2+\epsilon)}\left(v+\frac{a^2+b^2}{2}\right)-x\dot{Y},$$

where ϵ, v are the elliptic co-ordinates satisfying the equations

$$\frac{x^2}{a^2+\epsilon}+\frac{(y-Y)^2}{b^2+\epsilon}=1,\ \frac{x^2}{a^2+v}+\frac{(y-Y)^2}{b^2+v}=1,$$

and $$-b^2<\epsilon<\infty,\ -a^2<v<-b^2,\ b<a.$$

Example 2. Kirchhoff's Elliptic Vortex Cylinder.

x', y' are rectangular co-ordinates referred to axes rotating with uniform angular velocity $\dot{\omega}$ about their origin.

f, a, b are constants.

u', v' the components of the velocity parallel to the rotating axes.

$\zeta=\frac{1}{2}\left(\frac{dv}{dx}-\frac{du}{dy}\right)$=const. in this case ($u$, v are velocities parallel fixed rectangular axes in space).

It is shown that $u'=y'\left(\frac{2f}{b^2}-\dot{\omega}\right)$; $v'=-x'\left(\frac{2f}{a^2}-\dot{\omega}\right)$ satisfy the equations of motion.

The surfaces containing the same particles are the elliptic cylinders $\frac{x'^2}{a^2}+\frac{y'^2}{b^2}$=const.

If $\dot{\omega}=-\frac{2f}{ab}$ $\left(\text{which is}=\frac{2\zeta ab}{(a+b)^2}\right)$, then the motion may be supposed to be given by the above values of u', v' inside the cylinder

$$\frac{x'^2}{a^2}+\frac{y'^2}{b^2}=1,$$

and therefore to be rotational; but outside this it may be supposed irrotational, and its velocity potential

$$\frac{2\zeta ab}{a^2-b^2}x'y'\left(1-\frac{a^2+b^2+2\epsilon}{2\sqrt{(a^2+\epsilon)(b^2+\epsilon)}}\right)-\zeta ab\sin^{-1}\frac{x'}{\sqrt{a^2+\epsilon}}$$

where ϵ is the root of $\frac{x'^2}{a^2+\epsilon}+\frac{y'^2}{b^2+\epsilon}=1$, which lies between $-b^2$ and ∞, a being $>b$.

Example 3. With the same values for u', v' as in the last example, except, however, the relation between f and $\dot{\omega}$, it is shown that an irrotational motion continuous with the rotational motion inside the cylinder $\frac{x'^2}{a^2}+\frac{y'^2}{b^2}=1$ can exist between this and a confocal elliptic smooth rigid cylinder surrounding it; provided that confocal elliptic cylinder be made to rotate with the same angular velocity $\dot{\omega}$.

The ϵ of the confocal cylinder is—

$$=\frac{4\left(\frac{a^2+b^2}{a^2b^2}-\frac{\dot{\omega}}{f}\right)}{\left(\frac{\dot{\omega}}{f}\right)^2-\frac{4}{a^2b^2}}$$

where $-\infty<\frac{\dot{\omega}}{f}<-\frac{2}{ab}$.

The components of the velocity of this rotationally moving liquid are—

$$y'\left(\frac{2f}{b^2}-\dot{\omega}\right) \text{ and } -x'\left(\frac{2f}{a^2}-\dot{\omega}\right).$$

The components of the velocity of the irrotationally moving liquid are—

$$\frac{aby'}{a^2-b^2}\left[2\left\{f\left(\frac{1}{a^2}+\frac{1}{b^2}\right)-\dot{\omega}\right\}\sqrt{\frac{a^2+\epsilon}{b^2+\epsilon}}-\left\{\frac{4f}{ab}-\dot{\omega}\frac{a^2+b^2}{ab}\right\}\right]$$

$$\frac{abx'}{a^2-b^2}\left[2\left\{f\left(\frac{1}{a^2}+\frac{1}{b^2}\right)-\dot{\omega}\right\}\sqrt{\frac{b^2+\epsilon}{a^2+\epsilon}}-\left\{\frac{4f}{ab}-\dot{\omega}\frac{a^2+b^2}{ab}\right\}\right]$$

Example 4. The vortex sheets are coaxial circular cylinders, and the motion is everywhere perpendicular to the radius vector from the axis, and a function of the distance.

Thus radial velocity $=0$, velocity perpendicular radius vector $=-\mathrm{F}'(r)$.

This example is given merely to illustrate the expression of the components of the velocity in Clebsch's forms.

It is shown that

$$\lambda=\mathrm{F}(r).$$

$$\psi=-\left(t+\frac{r}{\mathrm{F}'(r)}\theta\right)\left(\mathrm{F}''(r)+\frac{1}{r}\mathrm{F}'(r)\right)$$

$$\chi=\theta\left\{\mathrm{F}(r)-r\mathrm{F}'(r)+\frac{r\mathrm{F}(r)\mathrm{F}''(r)}{\mathrm{F}'(r)}\right\}$$

$$+t\left\{\mathrm{F}(r)\mathrm{F}''(r)-\tfrac{1}{2}(\mathrm{F}'(r))^2+\frac{1}{r}\mathrm{F}(r)\mathrm{F}'(r)-\int\frac{dr}{r}(\mathrm{F}'(r))^2\right\}$$

If $\mathrm{F}(a)=0$, and the rotational motion be supposed confined to the interior of the cylinder $r=a$, then a suitable value of the velocity potential at external points is—

$$\phi=\theta(-a\mathrm{F}'(a))+t\left\{-\tfrac{1}{2}(\mathrm{F}'(a))^2-\int\frac{da}{a}(\mathrm{F}'(a))^2\right\}$$

Example 5. r, z being cylindric co-ordinates.

Z, an arbitrary function of the time; $\dot{\mathrm{Z}}$, its differential coefficient with regard to the time.

a, b, α positive constants.

τ the velocity from the axis of cylindric co-ordinates in the direction of r.

w the velocity parallel the axis of cylindric co-ordinates.

It is shown that $\tau=2ar(z-\mathrm{Z})$; $w=\dot{\mathrm{Z}}-2a(z-\mathrm{Z})^2-4b(r^2-\alpha^2)$ satisfy the equations of motion.

One set of the surfaces which always contain the same vortex lines is given by $ar^2(z-Z)^2+b(r^2-a^2)^2=\text{const.}$

If the constant be $< ba^4$, the equation represents ring-shaped surfaces.

The above expressions for the components of the velocity determine a possible case of rotational motion inside the hollow smooth rigid surface of annular form whose equation is $ar^2(z-Z)^2+b(r^2-a^2)^2=$ const., and which moves parallel to the axis of z with velocity $\dot{Z}$.

The values of χ, λ, ψ of Clebsch's forms of expression for the components of the velocity are calculated, but the writer has not succeeded in finding an irrotational motion outside one of the above annular vortex sheets continuous with the rotational motion inside it.

Could the solution be completed it would amount to a discussion of the motion of an annular vortex, all of whose parts are of finite dimensions.

Appendix.

The paper concludes with an Appendix, in which has been placed an account of Examples 1 and 2, the calculation of the potential of the elliptic cylinder $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$, the density of which at the point x, y, is

$$-\frac{c}{4\pi}\left(\frac{1}{b^2}-\frac{1}{a^2}\right)\frac{xy}{\left(\frac{x^2}{a^2}+\frac{y^2}{b^2}\right)^2}.$$

(Although this density is infinite along the axis of the cylinder, yet the total amount of matter in any elliptic cylinder surrounding the axis whose equation is $\frac{x^2}{a^2}+\frac{y^2}{b^2}=\text{const.}$, however small the constant may be, vanishes. Hence it is not singular that the potential is finite.)

The potential inside is—

$$\frac{c}{2}\cdot\frac{a-b}{a+b}xy+\frac{cab}{2}\left(\sin^{-1}\frac{\frac{x}{a}}{\sqrt{\frac{x^2}{a^2}+\frac{y^2}{b^2}}}-\sin^{-1}\frac{x}{\sqrt{x^2+y^2}}\right)$$

The potential outside is—

$$\frac{cab}{a^2-b^2}xy\left(\frac{a^2+b^2+2\epsilon}{2\sqrt{(a^2+\epsilon)(b^2+\epsilon)}}-1\right)+\frac{cab}{2}\left(\sin^{-1}\frac{x}{\sqrt{a^2+\epsilon}}-\sin^{-1}\frac{x}{\sqrt{x^2+y^2}}\right)$$

where $$\frac{x^2}{a^2+\epsilon}+\frac{y^2}{b^2+\epsilon}=1.$$

II. "Supplementary Note on the Constitution of Chlorophyll." By EDWARD SCHUNCK, F.R.S. Received February 6, 1884.

After the reading of the note on chlorophyll at the meeting of the Society on December 13th, I was reminded by Professor Stokes that he and others had succeeded in separating the complex to which the term chlorophyll had previously been applied into two substances, or rather groups of substances, one characterised by its green colour and red fluorescence, the other showing a more distinctly yellow colour without fluorescence, and he suggested to me that it would be advisable to ascertain whether the property of yielding glucose by decomposition with acids might not belong to one of these substances or groups of substances only. Professor Stokes at the same time kindly communicated to me the details of the process whereby he succeeded in effecting the separation referred to, a process depending on the action of carbon disulphide in removing some of the bodies contained in an alcoholic solution of crude chlorophyll in preference to others. The process employed for the same purpose by Mr. Sorby is essentially the same.

Before applying disulphide of carbon to an alcoholic extract of green leaves according to the process of Professor Stokes, it was necessary first to remove the ready-formed glucose, tannin, and other matters soluble in water, which almost always exist in such extracts, and which would by their presence have rendered the result of the experiment quite uncertain. This was done in the way I have already described. An ethereal solution of chlorophyll prepared by my method was evaporated, and the residue having been dissolved in alcohol, the solution was mixed with a quantity of carbon disulphide larger than the alcohol would dissolve, and the mixture well shaken. The carbon disulphide acquired a dark green colour, while the supernatant alcoholic liquid, containing principally the xanthophyll of Professor Stokes and Mr. Sorby, was yellow with a tinge of green. The two liquids having been separated, the lower dark green one was washed several times with alcohol to remove any of the xanthophyll that might still be present, and having then been mixed with a large quantity of alcohol, a current of air was passed through it to remove the excess of carbon disulphide as directed by Professor Stokes. In this way I obtained two liquids, one intensely green, the other deep yellow with only a tinge of green. The two liquids were found to contain substances essentially different so far as regards their products of decomposition with acids. The yellow liquid having been mixed with dilute sulphuric acid was evaporated in the water-bath, water being added during evaporation, until the liquid had lost nearly all its colour. A quantity of yellow fatty matter separated

during evaporation, and this having been filtered off the liquid was found to contain an abundance of glucose. The yellow fatty matter, insoluble in water, dissolved easily in alcohol, but the yellow solution showed none of the characteristic absorption bands of "acid chlorophyll." The dark green liquid, treated in exactly the same way, yielded a dark green product insoluble in water. The filtrate from this gave a slight reaction with Fehling's solution, but so trifling comparatively that I am inclined to attribute it to the presence of some substance not completely removed from the disulphide of carbon solution by washing with alcohol. The dark green product of the action of acid insoluble in water was soluble, though with difficulty, in boiling alcohol, the solution being dull green and showing the absorption bands due to "acid chlorophyll." If, therefore, chlorophyll be defined as the constituent of the green parts of plants, which gives a spectrum showing the well-known bands at the red end, and yields by decomposition with acids the product or products going by the name of "acid chlorophyll," of which Fremy's phyllocyanin is the most important and most characteristic, then chlorophyll is not a glucoside. The glucoside which accompanies it and resembles it as regards solubility in various menstrua may have to be sought among the group of bodies to which the generic name of xanthophyll has been applied.

February 14, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "On a New Reflecting Galvanometer of Great Sensibility, and on New Forms of Astatic Galvanometers." By THOMAS GRAY, B.Sc., F.R.S.E., and ANDREW GRAY, M.A., F.R.S.E. Communicated by Prof. Sir WILLIAM THOMSON, F.R.S. Received February 5, 1884.

The object of this paper is, first, to describe a galvanometer in which a principle which is, we believe, novel in its application to this particular purpose, has been employed to give an instrument of very great sensibility; and, second, to describe some forms of astatic galvanometers which are, we think, improvements on astatic combinations as ordinarily made and arranged for use.

Fig. 1 shows in elevation and horizontal section the coils and arrangement of needles adopted in an instrument of very great sensibility which we designed in the summer of 1882, and which with assistance from the Government Research Fund, we have had constructed for use in our experiments on the Electric Resistance of Glass and Allied Substances. It consists of two pairs of coils, *c c c c*, with hollow cores, arranged so that the axes of each pair are parallel and in a vertical plane. Each pair is carried by a vertical brass plate, and the two plates are inclined to one another at an angle of about 106°; and thus the vertical planes containing the axes of the coils are inclined to one another at an angle of about 74°. Two horseshoe magnets, *m*, *m*, made of steel wire of about 1 millim. in diameter, are connected together by a very light frame of aluminium, and are at such a distance apart as to hang, when the needle system is in equilibrium, with no current in the coils, freely within and nearly along the axes of the cores of the coils. The horseshoes are not plane, but are bent round so that they form approximately portions of one vertical cylinder of which the suspension-thread is the axis, and to which the axes of the coils form horizontal tangents near their middle points at the approximate positions of the poles of the needles. This form was given to the needles to lessen the chance of

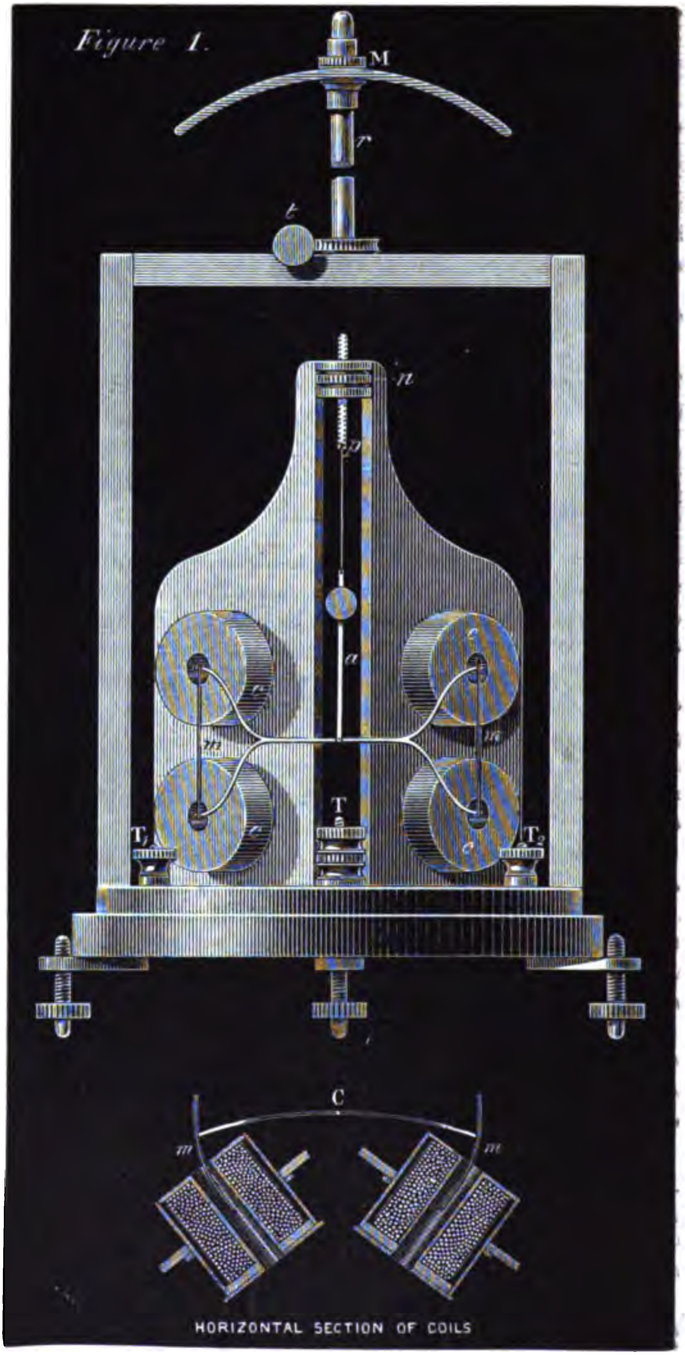


Figure 1.
M
r
t
n
p
a
m
m
T
T_1
T_2
C
m
m
HORIZONTAL SECTION OF COILS

their extremities coming into contact with the sides of the cores, in the case of large deflections; and it was intended to curve the axes of the coils similarly, to allow of the use of, as short as possible, a connecting-bar of aluminium. Straight cores are more easily made; but the diagram shows clearly that in this case, since it is advantageous to have the cores of the coils as small as possible, it is necessary to curve the needle. The needles are at present arranged with their unlike poles turned in similar directions, and are hung by a single fibre of unspun silk from the square screw-pin, *p*. A nut, *n*, working between two fixed projecting pieces raises or lowers *p*, and therefore the needles, without giving any twist to the thread.

The four coils are at present arranged in series, and have a combined resistance of 30,220 ohms made up of 5,392 yards of copper-wire of No. 50 B.W.G., laid on in 62,939 turns in all, approximately equally divided among the four coils. The distributing plate, described below, admits of ready joining of the coils in such a way as to diminish the sensibility or resistance of the instrument. When a current is sent through the instrument, the poles of one horseshoe are pulled farther into the corresponding coils, and the poles of the other horseshoe are pushed out of the coils. A couple tending to turn the system as nearly as may be round the axis of suspension is thus applied; and a mirror attached to the upright piece of aluminium, *a*, reflects a beam of light to a scale in the usual manner, to give a means of measuring the deflection. According to one arrangement of the instrument, the magnet M, carried in a horizontal position by the brass rod above the needles, is used to give a return couple, and to control the zero conveniently can be turned in azimuth by means of the tangent-screw *t*. The instrument is fitted with three terminals T, T_1, T_2, so that it can be used as described below as a differential galvanometer. These terminals and the connecting-wires are carefully insulated by vulcanite. As thus arranged we can easily give this instrument a sensibility so great as to give a deflection of one division ($\frac{1}{2}$-millim.) on a scale at a distance of rather more than a metre, with one Daniell's cell and a total resistance of 10^{11} ohms in direct circuit; a degree of sensibility which, we think, could not be reached with the most sensitive astatic instruments as ordinarily constructed. With special adjustments of the needles, we can obtain still greater sensibility, but the period of oscillation of the needles becomes then so great as to be troublesome when results have to be rapidly obtained. The instrument, however, has a very high sensibility for most purposes to which it can be applied, with at the same time a period of free vibration which is not inconveniently long. We have found it very convenient in our experiments on the resistance of glass at different temperatures, in the course of which, though not arranged for very high sensibility, it has been used with a battery

of about 120 Daniells to measure resistances of from 10^4 to 10^5 megohms.

To increase the rapidity with which observations can be taken, the instrument can be fitted with a vane immersed in a liquid; or placed in a nearly closed air-chamber of proper dimensions.

In another form of the instrument (fig. 2), of which an example is being completed for Sir William Thomson for use as a ballistic instrument, the four coils are arranged symmetrically in one plane, and a horizontal curved (slightly S or Z shaped) bar or frame of aluminium used to carry the vertical horseshoes. This frame passes through from one side to the other of the plate carrying the coils, so that one horseshoe enters its pair of coils from one side, and the other horseshoe its pair of coils from the other side. The coils are joined so that when a current is sent through the instrument both horseshoes are dragged further into their coils, or both pushed out at the same time. The needle system is thus turned round the suspension fibre, and the deflection is measured by means of a mirror and scale as before.

This form of the instrument has the advantage of being more compact and more easily constructed, and of admitting of readier adjustment of the magnets relatively to the hollow cores of the bobbins. The cores are made somewhat "trumpet-shaped" where the needles enter. Of course as the instrument which is being made is to be used for ballistic experiments, the cores have not been formed of metal tubes as in the instrument already described.

The chief advantage of the arrangement of coils and needles described above is that a great portion of the wire of the coils is placed very near to the poles of the needles, and in a very favourable position for exerting the electro-magnetic action required. The instrument, particularly the form shown in fig. 2, is very easily made, and does not cost more than an instrument of the ordinary kind. Of course a single horseshoe, or S or Z shaped bar, might be placed horizontally, and acted on by a pair of coils, and the principle thus applied to a single needle non-astatic instrument. In astatic instruments, however, of this form it is decidedly preferable, as shown below, to use vertical needles.

It is to be observed that if the line joining the poles or centres of gravity of magnetic polarity in each horseshoe be vertical, the system is perfectly astatic for a uniform field, for each vertical horseshoe is itself perfectly astatic. The pair of horseshoe needles can thus be adjusted to have as nearly as may be perfect astaticism in a uniform field, and thus made to preserve a nearly constant zero when under directive force, a result which it is exceedingly difficult to obtain in the ordinary arrangement of horizontal needles, and which certainly rarely exists when a horizontal magnet or magnets placed above or in an unsymmetrical position relatively to the needles is employed to

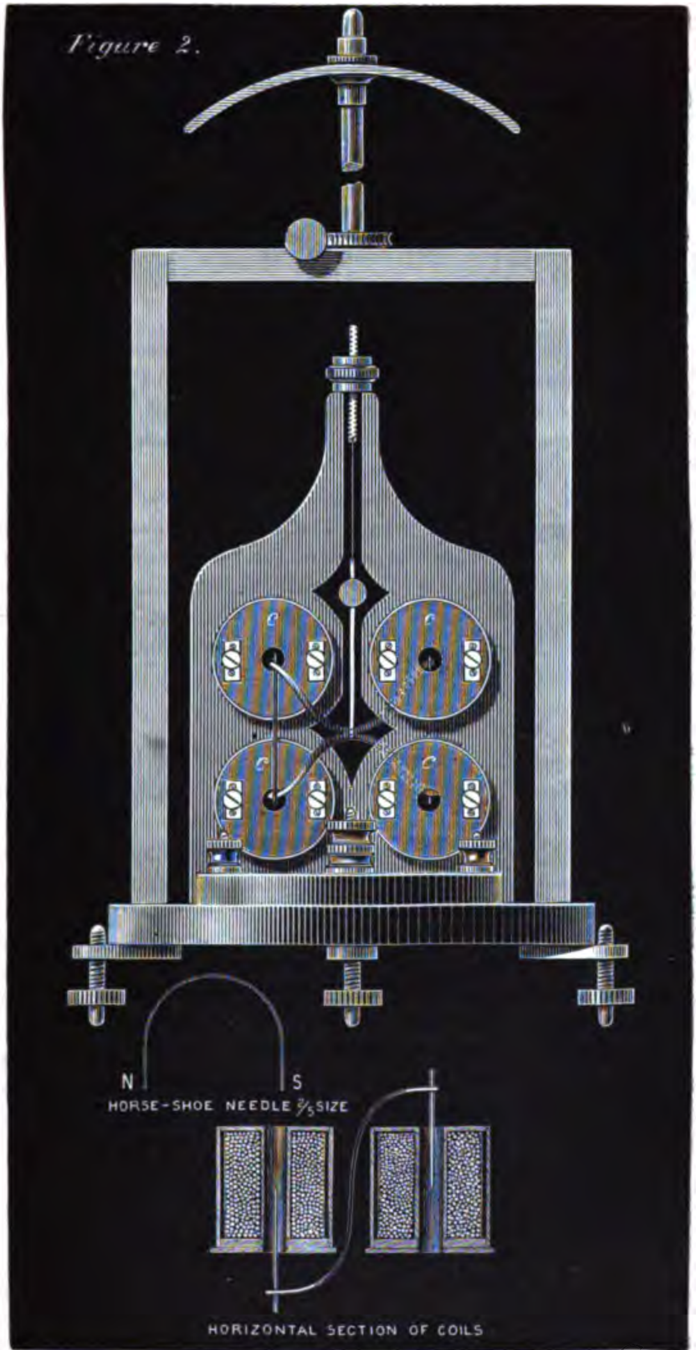


Figure 2.
C
C
C
C
N
S
HORSE-SHOE NEEDLE 2/5 SIZE
HORIZONTAL SECTION OF COILS

regulate the sensibility, as then one of the needles must be magnetized and the other demagnetized to a greater or less extent, depending on the position of the magnet. According to this latter arrangement, if we suppose the needles to be parallel or nearly so, and I to be the magnetic field intensity at the upper needle, I$'$ that at the lower needle in the same direction, m the magnetic moment of the upper needle, m' that of the lower needle, C the current flowing, θ the deflection produced, and K a constant, we have—

$$C=K\frac{Im-I'm'}{m+m}\tan\theta.$$

Slight changes in m or m' or in both may, therefore, affect the constant of the ordinary instrument very seriously, and as a matter of fact its constant has to be continually redetermined, and instead of preserving a practically constant zero, it is very sensitive to magnetic disturbances in the neighbourhood.

In the case, however, of needles adjusted to be accurately vertical these disadvantages do not exist. The needles cannot be affected in the same way by directing magnets and retain their astaticism for uniform field, I, I$'$ being the horizontal field intensities at the upper and lower extremities of the needles, C the current strength, θ the deflection of the needles, and K a constant depending on the coils, we have approximately—

$$C=K(I-I')\sin\theta.$$

The sensibility of the instrument can, therefore, be increased to any desired extent by placing the magnet M at a greater distance from the needles (or by counteracting its action by a smaller magnet placed nearer to the needles) so as to make $I-I'$ sufficiently small. Further, variations of the strength of the horseshoe needles produce no effect unless they consist of changes of magnetic distribution, which may produce a deviation from perfect astaticism. When the instrument is properly adjusted and the needles are as nearly as possible uniformly magnetized, but little disturbance of this kind can be produced by the magnetizing action of the coils, since both poles of each have their magnetism augmented or both diminished at the same time in the arrangement of fig. 2, or both poles of one are magnetized more intensely in some degree, and both poles of the other weakened in the arrangement of fig. 1.

Another possible arrangement of such a system of needles is with like poles turned in similar directions. The system will still be perfectly astatic if properly adjusted; and to give a return couple towards a zero position a magnet may be used, placed, for example, horizontally in the vertical plane at right angles to the front of the instrument, in a line passing through the suspension thread. If this

magnet be placed nearer to say the lower ends than the upper ends of the needles, and the polarity of the end turned towards the needles be of the *same* name as that of the nearer ends of the needles, they will have a position of stable equilibrium when no current is flowing, with a horizontal line joining a pole of each needle at right angles to the direction of the magnet. The accurate law of variation of deflection with current is, however, in this case more complicated, and the instrument in some cases might have to be graduated by experiments with known currents of different amounts. Any change also of the magnetic distribution of the controlling magnet would affect the indications of the instrument.

We propose in some cases to hang the needle system in a uniform field, and to use a small needle rigidly connected with it, but placed so as not to be perceptibly affected by the coils, to give directive force to the magnetic system. This arrangement, of course, would not be astatic, but would give great sensibility on account of the leverage of the horseshoe needles as arranged.

Thus if m denote the magnetic moment of the small needle, H the horizontal component of the earth's magnetic force, k a constant depending on the coils, ϕ the strength of pole of each of the horseshoes (supposed of equal strength), and d the distance of these poles from the suspension thread, we have, since the deflection is small, for the turning couple exerted by the coils $4\mathrm{C}k\phi d$, and for the return couple $m\mathrm{H}\theta$, and therefore—

$$\mathrm{C}=\frac{m\theta}{4k\phi d}.$$

Of course this arrangement is applicable whether like or unlike poles are turned in similar directions. It has the disadvantage that any change of m or ϕ or of both would affect the constant of the instrument.*

The sensibility of any of these arrangements might also be increased by bringing out a very light arm, say from the middle of the cross bar connecting the horseshoes, or from any other convenient point, and hanging the mirror by means of a bifilar, one thread of which

* *Note added March* 20, 1884.—We propose to mount this small needle in such a way that it can be turned round a horizontal axis at right angles to its length, and also round a vertical axis, so that by means of this needle alone both the sensibility and the zero of the instrument can be adjusted. When the galvanometer is not intended for ballistic experiments, the frame on which the small needle is mounted may conveniently be immersed in a liquid and made to act as a vane for bringing the needle system quickly to rest.

It is to be observed that, in consequence of the horseshoe needles being placed in these instruments at a considerable distance from the axis of suspension, a very small value of I−I′ is sufficient to give the needle system such a directive force as to prevent any great error due to the rigidity or the viscosity of the suspending fibre.

is attached to the outer extremity of this arm, and the other to a near fixed point. The distance between the fibres being small in comparison with the length of the arm, small deflections would be greatly multiplied. This device would, no doubt, render a greater degree of skill and delicacy of manipulation necessary in the operator or experimenter, but we think it or some similar plan might in some cases be adopted, and the construction of these instruments renders its application to them very easy.

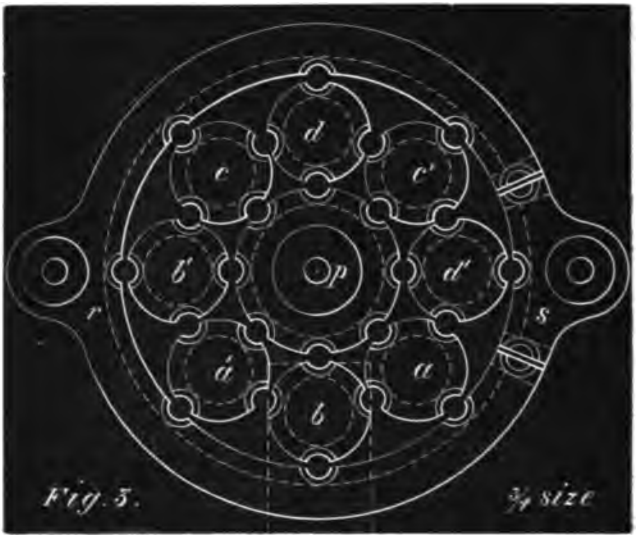


Fig. 3 shows a form of distributing plate which we have devised for use with these instruments, or any other in which coils are joined sometimes in multiple arc and sometimes in series, or partly in both ways. It consists of a set of (for those instruments described in this paper, which have four coils) eight brass pieces a, b, a', b', c, d, c', d', arranged as shown round a central piece p and within an outer ring r, all carried by the vulcanite base plate of the instrument. Each of these brass pieces a, b, &c., can be connected with p or r, or with either of the two adjacent pieces by means of plugs. The ring r is divided as shown on each side of the piece s, and each of these divisions can be bridged across by means of a plug when necessary; r, p, and s are each provided with a screw terminal, which it is useful to have for some applications of the plate. The connexions are as follows:—Suppose the coils on one side to be distinguished by the letters α, β, and those on the other side by γ, δ, and a current to be flowing through all the coils so as to produce a conspiring action in each. Now call the terminal of the coil α, by which the current

enters α_a, and that by which it leaves α_a', the terminal by which the current enters β, β_b, and that by which it leaves β_b', and so on. Then the piece a and α_a are in connexion, similarly a' and α_a', and so on for all the pieces and terminals. The central piece p is connected to the middle terminal of the instrument, the ring r to one of the other terminals, and the piece s to the remaining terminal.

The moveable or plug-connexions for joining the coils in series, or in any other desired way, are now evident. For series, p is connected by plug to a, a' to b, b' to c, c' to d, and d' to r. The current thus enters by the middle terminal, passes round all the coils in the proper direction and emerges by the terminal connected with r, and *vice versâ* for the reverse deflection. When the instrument is thus joined, the piece s is generally connected by a plug with r, so that the two side terminals of the galvanometer form one common electrode of the instrument. A spare terminal-screw is thus available for one electrode of a shunt-box, or for any other purpose. If it is desired to join the pair of coils α, β on one side in multiple arc, and in the same way γ and δ on the other side, and to connect these two pairs in series a and b are connected together and to p by plugs, then a', b', c, d together, and finally c' and d' together and to r or s.

To join all the coils in multiple arc a, b, c, d are joined to p by plugs, and similarly a', b', c', d' to r or s.

If it is desired to use the instrument as a differential galvanometer a may be joined to r, a' to b, and b' to p: again s joined to d' (the divisions between s and r being unplugged), d to c', and c to p. A current then may be supposed to pass from one side terminal through the coils α, β, tending to turn the needle system in one direction, and at the same time a current from the other side terminal of the instrument through the coils δ, γ, tending to turn the needle system in the opposite direction, the two currents meeting in the middle terminal.

This distributing plate can be joined so as to effect any other desired arrangement; and the same general plan of plate is applicable to any galvanometer with multiple coils, or with coils wound in sections.

Lastly, we desire to propose one or two modifications of the instruments described above. Instead of a set of four coils with hollow cores and horseshoe needles as described, eight coils might be used—one set of four arranged in rectangular order in a vertical plane facing a second set of four similar coils in a parallel plane at a small distance from the first. Two *straight* needles of thin steel wire connected together as rigidly as possible by very light bars of aluminium, are so chosen as to length and so arranged that they hang from a single silk fibre with their lengths vertical and a magnetic pole as nearly as may be in the line joining the centres of each mutually opposite pair of coils. A magnet giving a differential field at the

needles, if their like poles are turned in dissimilar directions, or any other arrangement may be used, and a current is sent through the coils in any desired way by means of a distributing plate or otherwise.

We think that for cable-testing instruments of high resistance, or for ballistic instruments, an instrument of this form, or of the following still simpler form, might be found advantageous.

Astatic galvanometers of Sir William Thomson's pattern are usually made with two coils, one above the other, split into four by a narrow vertical space in which the needle system is suspended, and which admits of the ready removal of the needles for adjustment. We propose to hang in this space, in a plane nearly parallel (when no current is flowing) to the two coils, two thin magnetic needles of steel wire side by side, kept with their lengths accurately vertical, and at a short distance apart (say $\frac{1}{4}$ or $\frac{3}{8}$ of an inch) by light aluminium, or other non-magnetic bars. Such a system of needles with unlike poles turned in similar directions would plainly experience a similar electromagnetic action to that exerted by the coils on the needles in the ordinary so-called astatic combination. But two straight vertical needles would plainly be perfectly astatic in a uniform magnetic field; and this astaticism for uniform field would not be liable to disturbance from any arrangement of magnets applied to give directive force to the system, as for example one or more magnets directing the system by means of a more powerful action at one end of the needle system than at the other as shown in Figs. 1 and 2, or magnets arranged symmetrically with respect to both ends of the needles. An instrument with such a system of needles ought therefore to be subject to but slight, if any, disturbance in ordinary circumstances of sensibility when masses of steel or iron are being moved about at some little distance, and would we think be found useful in such cases, as for example in cable testing rooms.

We may, perhaps, be allowed to state in conclusion that with the instrument described above and shown in Fig. 1, we have been able to measure the resistance at temperatures of from 100° to 150° C. of small globular flasks (about 3 inches in diameter) of good flint glass of considerable thickness. Some of these flasks have been analysed under the superintendence of Mr. James J. Dobbie, D.Sc., Assistant to the Professor of Chemistry in the University of Glasgow. Dr. Dobbie has joined us in our experiments on the Resistance and Specific Inductive Capacity of Glass and Allied Substances; and we have in preparation a joint paper embodying the results of our preliminary experiments, which we hope soon to present to the Royal Society.

II. "A New Form of Spring for Electric and other Measuring Instruments." By Professors W. E. Ayrton, F.R.S., and John Perry, M.E. Received February 6, 1884.

In steam and gas engine indicators, the pressure of the fluid on a piston produces a slight shortening of a spiral spring which is magnified by a lever, and so the pressure of the steam or gas is recorded. In what are usually known as spring balances there is also occasionally a magnification of the elongation of a spiral spring effected by the use of a rack and pinion. Such magnifying arrangements, however, not only introduce inaccuracy by the bad fitting of hinges or of teeth, an inaccuracy which is aggravated by wear, but they increase the cost of measuring instruments and their liability to get out of order.

And, as an example of the difficulty of using a wheel and pinion for the magnification of an angular motion produced by a small force, we may mention that although we used this plan for a year or more in our electric measuring instruments, and although the wheels and pinions were made by a good watchmaker, still the friction involved in such a plan has induced us to abandon it in favour of the new arrangement which is the subject of this communication.

The telescopic method employed by Weber, and the spot of light method due to Sir W. Thomson for magnifying the effect of an angular motion are, of course, unequalled for stationary measuring instruments, but for instruments which must be carried about and used quickly without the necessity of adjustment, these most ingenious reflecting methods are quite unsuitable.

With an ordinary cylindric spring, having a small angle between the osculating plane and a plane perpendicular to the axis, as is the case with all spiral springs such as are in practical use, and of which fig. 1 is an example, it is well known that but very little rotation is produced between its ends by the application of an axial force. Consequently with such springs, it is only possible to obtain magnification by the employment of a system of levers, or of a rack and pinion. It occurred to us, therefore, to consider whether it would not be possible to make a spiral spring of such a nature that for a comparatively small axial motion of its ends there should be considerable rotation of one end relatively to the other, and by the employment of which all levers, racks, and pinions could be dispensed with, so that no error could be introduced by wear and tear, or by want of fitting of joints, and further so that the temperature correction should be merely one affecting the rigidity of the material used as a spring, and not a correction such as has to be applied in consequence of the contractions and expansions of the various parts of an ordinary magnifying apparatus.

In order to ascertain this fact, it is necessary to consider what are the general laws governing the behaviour of spiral springs. Let the centres of all cross sections of the wire, or strip, forming the spring lie on a right circular cylinder of radius r, let the spiral have everywhere an inclination α to the plane perpendicular to the axis of the cylinder, and let a force F act at one end of the spring along the axis, the other end of the spring being fixed.

In the cross section of the wire at any point P (fig. 2), we have, due to the axial force F, tensile stress whose effect in deforming the spring may be neglected in comparison with the other effects to be described, and we have the stresses produced by a couple Fr acting about the axis PM. PS, PM, and PU are all in a plane tangential to the cylinder at the point P, PM being drawn in this plane perpendicular to the axis of the cylinder, PU tangential to the spiral line, and PS perpendicular to PU.

This couple is equivalent to the couple $Fr\cos\alpha$, which is a twisting couple about the axis PU, and to $Fr\sin\alpha$, a bending couple about the axis PS. If now B is the flexural rigidity of the wire in the osculating plane, and if A is the torsional rigidity about the spiral line at P we have—

$$\frac{Fr\cos\alpha}{A}$$

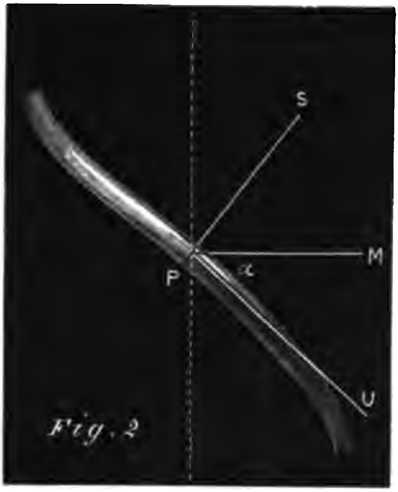


Fig. 2

as the angular twist per unit of length about the axis PU, and

$$\frac{Fr \sin \alpha}{B}$$

as the angular bending per unit of length about the axis PS.

Resolving both of these horizontally and vertically, we have for the total angular motion in a vertical plane, that is, about the axis PM—

$$Fr\left(\frac{\cos^2 \alpha}{A}+\frac{\sin^2 \alpha}{B}\right),$$

and for the angular motion in a horizontal plane—

$$Fr \sin \alpha \cos \alpha\left(\frac{1}{A}-\frac{1}{B}\right).$$

If the angular motion, in a horizontal plane, of the free end of the spring relatively to the fixed end be called ϕ, and if the axial increase of length be called d, and the whole length of the spring along the spiral l, then—

$$\phi=lFr \sin \alpha \cos \alpha\left(\frac{1}{A}-\frac{1}{B}\right) \quad . \quad . \quad . \quad . \quad . \quad (1),$$

and

$$d=lFr^2\left(\frac{\cos^2 \alpha}{A}+\frac{\sin^2 \alpha}{B}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

The theory of the strength and stiffness of the ordinary cylindric spiral spring for small angles was given, we believe for the first time, by Professor James Thomson, in the "Cambridge and Dublin Math. Journ.," for 1848, and in "Thomson and Tait's Natural Philosophy,"

vol. i, §§ 588–608, the general behaviour of wires subjected to forces at their ends is investigated, and we might have obtained equations (1) and (2) from the general result of that investigation given in Article 607, but we have preferred to deduce these two equations from first principles.

The general expression worked out by M. de St. Venant for A for a prism of rectangular section involves an infinite series, and its use would give rise to mathematical expressions of an unwieldy character in an investigation like the present. We have, therefore, decided to consider our strip of which the new spring is to be made as having an elliptic section. We may mention, however, for the benefit of students, that an examination of the expression given by Thomson and Tait, "Nat. Phil.," vol. i, for the torsional rigidity of a prism of rectangular section has led us, we believe, to detect two errors in it; one is a misprint of nab^2 for nab^3, the other that the coefficient employed $\left(\frac{2}{\pi}\right)^5\frac{b}{a}$ ought to be $\left(\frac{2}{\pi}\right)^5\frac{2b}{a}$. A friend of ours who has been kind enough, at our request, to check the investigation agrees with us in thinking that there are two errors, but considers that the second is not in the coefficient but in the fact that $\frac{1}{3}$ has been used in the formula in place of $\frac{1}{6}$. We still are of opinion, however, that the error is in the coefficient, because when we employ our two corrections, we obtain—

$$A=nab^3\left[\frac{1}{3}-\left(\frac{2}{\pi}\right)^5\frac{2b}{a}\Sigma\frac{1}{(2i+1)^5}\frac{1-\epsilon^{-(2i+1)\frac{\pi a}{b}}}{1+\epsilon^{-(2i+1)\frac{\pi a}{b}}}\right],$$

where a and b are the length and breadth of the strip, and this formula we find agrees with the experimental results of M. de St. Venant, and also with the calculated numbers given in Thomson and Tait's "Natural Philosophy," § 709, for the torsional rigidity of a square shaft; whereas neither the formula for A given in Thomson and Tait's book, nor the formula as corrected by our friend, will do this.*

Reverting to the elliptic section, if the principal semi-diameters of this ellipse are b, measured in the osculating plane of the spiral, and a measured perpendicular to this plane, the values of the two rigidities are—

$$A=\frac{N\pi a^3b^3}{a^2+b^2},\text{ and }B=\frac{E\pi ab^3}{4},$$

where N is the modulus of rigidity of the material, and E is Young's modulus.

Hence substituting, equation (1) becomes—

* Since this paper was presented Mr. R. T. Glazebrook has been kind enough to go through the investigation, and he confirms our correction of the formula as given in Thomson and Tait's "Natural Philosophy."

$$\phi = l\mathrm{F}r \sin\alpha \cos\alpha \frac{1}{\pi ab^3}\left(\frac{a^2+b^2}{\mathrm{N}a^2} - \frac{4}{\mathrm{E}}\right) \quad . \quad . \quad . \quad . \quad (3),$$

$$d = \frac{l\mathrm{F}r^2}{\pi ab^3}\left(\frac{a^2+b^2}{\mathrm{N}a^2}\cos^2\alpha + \frac{4}{\mathrm{E}}\sin^2\alpha\right) \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Maximum Rotation, Positive and Negative Springs.—If we merely require that ϕ shall be a maximum for a given axial force F, independently of the stresses produced in the material, and independently of the axial elongation, then it is evident that α ought to be equal to 45°, and l and r as great as possible. If our other condition is that the area of cross section of the strip is to be a constant—

Let $$ab = s,$$

and $$a = kb,$$

so that $$b^2 = \frac{s}{k}.$$

Then $$\frac{1}{\pi ab^3}\left(\frac{a^2+b^2}{\mathrm{N}a^2} - \frac{4}{\mathrm{E}}\right) = \frac{1}{\pi k\frac{s^2}{k^2}}\left(\frac{k^2+1}{\mathrm{N}k^2} - \frac{4}{\mathrm{E}}\right),$$

$$= \frac{1}{\pi s^2}\left(\frac{k}{\mathrm{N}} + \frac{1}{\mathrm{N}k} - \frac{4k}{\mathrm{E}}\right),$$

or since N is equal usually to about two-fifths E, we find that

$$\phi \propto \frac{5}{k} - 3k.$$

Now when k is very small this is very great. On the other hand, when k is very great this is also very great, but of the opposite sign. As k increases from 0 the expression to which ϕ, or the amount of turning, is proportional diminishes until when k reaches a value nearly 1·3, ϕ becomes 0, or there is no rotation of the spring produced by an axial force. In fact, for small values of k there is a rotation of the spring in the direction of the coiling, and the amount of rotation becomes the greater the smaller k is, while for values of k greater than 1·3 there is a rotation of the spring opposite to the direction of the coiling, the rotation becoming greater the larger k is.

Fig. 3 shows a spring in which k is greater than 1·3, and it is found that there is an uncoiling on the application of an axial force. Fig. 4 shows a spring made of the same material, but the wire has been passed through rolls so as to flatten it in the opposite way, and now a rotation tending to coil it up is found to be produced by the application of an axial force.

The twisting torque to which the spring is subjected is $\mathrm{F}r\cos\alpha$, and the bending torque to $\mathrm{F}r\sin\alpha$. But the twist must be multiplied by $\sin\alpha$, and the bend by $\cos\alpha$ when we project these motions on a horizontal plane. So far then as the total rotation in a horizontal plane of the free end of the spring relatively to the fixed end is concerned, it may be regarded as being produced by equal twisting and bending torques, each of them equal to $\mathrm{F}r\sin\alpha\cos\alpha$; and the total rotation of the free end of the spring relatively to the fixed end, which is the special feature of the springs we are considering, is proportional to the difference between the two angular rotations produced in the wire by these equal bending and twisting torques. The twist alone would cause an increase in the number of coils, that is, a rotation in the direction of coiling which is what we call positive, while the bending, or rather the unbending, alone would cause a negative rotation, or one tending to uncoil the spring. When both occur together in the actual spiral spring subjected to an axial force the total rotation is positive or negative, according as the angular twist or the angular bend is the greater. Hence the flexural and torsional rigidities of the wire alone determine whether the rotation is positive or negative.

It is well known, for example, that when a wire of circular section is subjected to equal twisting and bending torques the twist is greater than the bending for almost all substances, that is, substances in which the ratio of the modulus of rigidity to Young's modulus is

between one-third and one-half. Hence we may expect that in a spring made of round wire, and with the spires making an angle of 45° with a plane perpendicular to the axis, the total rotation will be positive for an axial force applied so as to lengthen the spring. And experiment shows that this is the case.

If the wire be flattened and bent so that the flat side of the strip touches the cylinder on which the wire is coiled, as shown in fig. 3, then the arrangement is such that the bending is greater than the twist. Hence an axial force applied so as to lengthen this spring causes a negative rotation, whereas if the strip be coiled as in fig. 4, so that the edge of the strip lies against the cylinder on which it is coiled, an axial force similarly applied will now cause a positive rotation. It is almost certain that for any strip of material the positive value of ϕ obtained with the latter form of spring is likely to be greater than the corresponding negative value with the former kind, but the difficulty of manufacturing the second form of spring, where k has a very small value, has compelled us to confine our attention to the former type.

Permanent Set.—Having constructed some very delicate springs of this kind, one of the first difficulties which we met with arose from their liability to acquire a permanent set, so that it has been necessary to determine the dimensions of the spring which will give the largest amount of rotation with the minimum amount of stress in the material.

The shear stress at any point x, y in an elliptic section if the prism has received a twist τ per unit length, is—

$$N\left(\tau x+\frac{d\gamma}{dy}\right) \text{ in the direction of } y,$$

and

$$N\left(-\tau y+\frac{d\gamma}{dx}\right) \text{ in the direction of } x,$$

where

$$\gamma=-\tau\frac{a^2-b^2}{a^2+b^2}xy,$$

where a and b are the principal semi-diameters of the ellipse, the axis of y being parallel to the semi-diameter b.

Consequently these shear stresses equal—

$$\frac{2b^2N\tau x}{a^2+b^2} \text{ and } -\frac{2N\tau y a^2}{a^2+b^2} \text{ respectively;}$$

and the total shear stress is equal to

$$\frac{2N\tau}{a^2+b^2}\sqrt{b^4x^2+a^4y^2}.$$

From this we see that the greatest shear stress is at the ends of the minor axis, and at the boundary is the least at the ends of the major axis—

If C is the twisting couple we know that—

$$\tau=\frac{C}{A}, \text{ or}$$

$$\tau=\frac{C}{N}\frac{a^2+b^2}{\pi a^3b^3}.$$

Using these values of τ in the above we obtain for the total shear stress at a point x, y of an elliptic section—

$$\frac{2C}{\pi a^3b^3}\sqrt{b^4x^2+a^4y^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

and this at the extremity of the minor axis becomes

$$\frac{2C}{\pi ab^2}.$$

Applying (5) to our case where the twisting couple is $Fr\cos\alpha$, and the bending couple $Fr\sin\alpha$, and recollecting that the direction we have chosen for y on the section is perpendicular to the axis of the spiral, we see that at a point x, y on the section bent about the axis of x, that is, x is perpendicular to the plane in which bending occurs the shear stress q equals—

$$\frac{2Fr\cos\alpha}{\pi a^3b^3}\sqrt{b^4x^2+a^4y^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

and the tensile or compressive stress p at the point x, y due to bending equals

$$\frac{4y}{\pi ab^3}Fr\sin\alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

And the resultant tensile, or compressive, stress f at the point x, y is

$$\frac{p}{2}+\sqrt{\frac{p^2}{4}+q^2},$$

Therefore since

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$$

is the equation of the ellipse, the stress at the boundary,

$$f=\frac{2Fr}{\pi ab^3}\left(y\sin\alpha+\sqrt{y^2+\cos^2\alpha\frac{b^2}{a^2}(b^2-y^2)}\right) \quad . \quad . \quad . \quad (8).$$

We have now to consider for what value of y this expression is a maximum. The possible values of y are between 0 and b; and we see that if $1-\frac{b^2}{a^2}\cos^2\alpha$ is positive, f will be greatest when y has its greatest possible value, namely b. For sections 1, 2, 3, 4, therefore, of the strip shown in fig. 5, where AB is the axis of the spring,

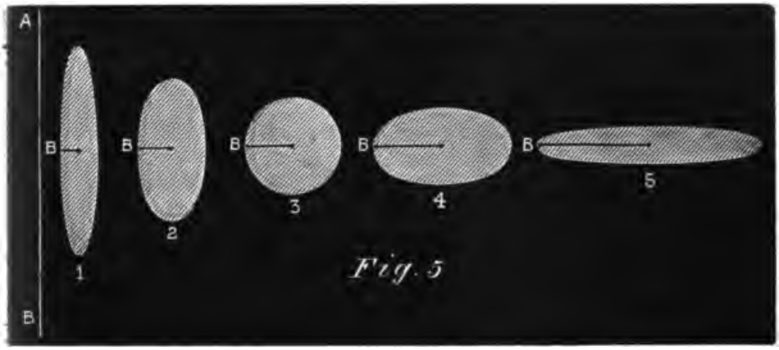


Fig. 5

the greatest stress will in each case be at B. If, however, b be very large compared with a, which is the case in the section 5, fig. 5, then f will not have its greatest value at B.

We find also that when

$$a<b\cos\alpha,$$

f has its greatest value when

$$y=\frac{ab^2\sin\alpha}{\sqrt{(b^2\cos^2\alpha-a^2)(b^2-a^2)}} \quad . \quad . \quad . \quad . \quad . \quad (9).$$

Now this value of y is greater than b so long as

$$a^2b^2\sin^2\alpha>(b^2\cos^2\alpha-a^2)(b^2-a^2);$$

and when these are equal we have

$$a^2=b^2(1\pm\sin\alpha)$$

where the positive sign is evidently inadmissible, since our condition above is a less than b.

Hence from b equal 0 through b equal a to b equal $\frac{a}{\sqrt{1-\sin\alpha}}$, the greatest stress occurs at the extremity of the y axis. After that, the point of greatest stress is nearer to a, but still on the circumference of the ellipse.

Now if b is very great compared with a, then we have as a limit,

$$y=a\tan\alpha,$$

so that the greatest stress can never occur at the end of the semi-diameter a, but may be very near to it.

On the supposition then that, relatively to a, b has any value from 0 to $\frac{a}{\sqrt{1-\sin\alpha}}$, the greatest stress in an elliptic section occurs at the end of the semi-diameter b, which is parallel to the axis of y, and this is the case we will first confine ourselves to.

Maximum Rotation in Relation to Permanent Set.—If, now, f is the greatest stress at any point of the section,

$$\frac{\phi}{f}=\frac{1}{2}\frac{l}{b}\frac{\sin\alpha\cos\alpha}{1+\sin\alpha}\left(\frac{a^2+b^2}{\mathrm{N}a^2}-\frac{4}{\mathrm{E}}\right)\quad . \quad . \quad . \quad . \quad . \quad (10).$$

The conditions that make this a maximum are those which for a given axial force applied to our spring produce the greatest amount of turning of the free end with the least amount of stress on the material, and therefore with the least chance of permanent set. And as regards the value of α, it is clear that

$$\sin\alpha=-\tfrac{1}{2}\pm\tfrac{1}{2}\sqrt{5},$$

or

$$\alpha=38^\circ\ 10',$$

will give the greatest value.

Maximum Rotation compared with Axial Motion.—From equations (3) and (4) we have

$$\frac{\phi}{d}=\frac{\tan\alpha}{r}\times\frac{\frac{a^2+b^2}{\mathrm{N}a^2}-\frac{4}{\mathrm{E}}}{\frac{a^2+b^2}{\mathrm{N}a^2}+\frac{4}{\mathrm{E}}\tan^2\alpha}\quad . \quad . \quad . \quad . \quad . \quad (11),$$

and the conditions that make this a maximum are those which for a given axial force applied to the spring give the greatest amount of turning of the free end of it with the least amount of axial lengthening. As regards the value of α, it can easily be shown that $\frac{\phi}{d}$ will be a maximum when

$$\tan\alpha=\sqrt{\frac{\mathrm{E}}{\mathrm{N}}\cdot\frac{a^2+b^2}{4a^2}}.$$

If b is small in comparison with a, which is a condition, as already explained, we are led by facility of construction to adopt, then

$$\tan\alpha=\tfrac{1}{2}\sqrt{\frac{\mathrm{E}}{\mathrm{N}}}.$$

As a rough approximation, taking N the modulus of rigidity at two-

fifths of Young's modulus, the ratio given by M. St. Venant, this value of $\tan \alpha$ becomes equal to $\sqrt{\cdot 625}$, or

$$\alpha = 38^\circ\ 19',$$

makes $\frac{\phi}{d}$ a maximum.

We have already seen, from (3), that to merely make ϕ a maximum for a given axial force, α ought to be 45°, and l and r as great as possible.

We are therefore led from three considerations—

1. That an axial force shall produce a maximum relative rotation;
2. That the rotation shall be great without producing permanent set in the material;
3. That the rotation shall be great in comparison with the elongation of the spring;

to make the angle of the spiral about 45°.

Further, to produce the first two of these conditions, it will be observed that the length of the wire forming the spring ought to be great, while the third condition is independent of the length.

Next, with regard to the proper radius r to give to the coils of the spring. ϕ increases with r, $\frac{\phi}{f}$ is independent of r, and $\frac{\phi}{d}$ varies inversely as r. Hence, as the first and third conditions are antagonistic one to the other, the value of r must be chosen to suii the conditions of the instrument in which the spring is to be used; that is, we must consider in any special case whether the possibility of permanent set or a large axial motion is more to be avoided in the particular instrument in question.

Let us now consider how ϕ, $\frac{\phi}{f}$, and $\frac{\phi}{d}$ depend on a and b. Referring to equation (3), if b is not greater than a, it is clear that the smaller b is the larger ϕ will be. Next putting N equal to $\frac{2}{5}$ E, an approximation sufficiently accurate for the substances likely tc be employed, we see from equation (10) that $\frac{\phi}{f}$ depends on

$$-\frac{1}{b\mathrm{E}}\left(1\cdot 5-\frac{5b^2}{2a^2}\right),$$

and as in this first case we are limited to values of b between 0 and $\frac{a}{\sqrt{1-\sin\alpha}}$, or between 0 and about 1·84 a if we put α equal to 45°, a value not far from that which we have already determined to be the best, then it is obvious that the smaller b is the larger will be $\frac{\phi}{f}$.

From equation (11) we see that $\frac{\phi}{d}$ depends on

$$\frac{(a^2+b^2)\mathrm{E}-4\mathrm{N}a^2}{(a^2+b^2)\mathrm{E}+4\mathrm{N}a^2};$$

or, putting N equal to $\frac{2}{5}$ E, this becomes

$$\frac{5\frac{b^2}{a^2}-3}{5\frac{b^2}{a^2}+13}.$$

Hence, remembering as before that b^2 has only values between 0 and $3{\cdot}4a^2$, we see that the last fraction varies between $-0{\cdot}23$ and $+0{\cdot}47$.

Hence, taking b small, which gives the greatest values for ϕ and $\frac{\phi}{f}$, gives a value for $\frac{\phi}{d}$ not less than half as great as if we had taken the largest possible value of b.

Finally, therefore, if b varies from 0 to $\frac{a}{\sqrt{1-\sin\alpha}}$, the best practical value of b is an extremely small one, or the strip should be wound as in figure 3.

If b is greater than $\frac{a}{\sqrt{1-\sin\alpha}}$, conditions other than those given above may make ϕ, $\frac{\phi}{f}$, and $\frac{\phi}{d}$ have their maximum values; but, since the difficulty of manufacturing metal springs of the form shown in fig. 4 must necessarily render their employment but very limited, a mathematical examination of this problem has not much practical value. We therefore merely mention that in this case calculation shows that

$$\frac{\phi}{f}=\tfrac{1}{2}la\tan\alpha\left(\frac{a^2+b^2}{\mathrm{N}a^2}-\frac{4}{\mathrm{E}}\right)\frac{1}{b^2}\sqrt{\frac{b^2\cos^2\alpha-a^2}{b^2-a^2}} \quad . \quad . \quad (12),$$

where f is the maximum stress anywhere in the section, the maximum stress in this case not occurring at the extremity of the b diameter.

The general conclusions therefore arrived at are, that in order, with a given axial force to obtain a large amount of turning of the free end of the spring, combined with small maximum total stress in the material, and not too much axial motion of the free end of the spring, the strip of elliptic section should be as long and as thin as possible, should be wound in a spiral such that the osculating plane makes an angle of 40° to 45° with a plane perpendicular to the axis of the spiral, and so that the smaller diameter of the elliptic section is at right angles to the axis of the spiral.

Under these circumstances, $2a$ being the major diameter and $2b$ the minor diameter of the ellipse,

$$\phi=\frac{2l\mathrm{F}r\sin 2\alpha}{\pi ab^3}\left(\frac{1}{4\mathrm{N}}-\frac{1}{\mathrm{E}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

f the maximum stress at a section

$$=\frac{2\mathrm{F}r}{\pi ab^2}(1+\sin\alpha) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

and

$$d=\frac{l\mathrm{F}r^2}{\pi ab^3}\left(\frac{\cos^2\alpha}{\mathrm{N}}+\frac{4\sin^2\alpha}{\mathrm{E}}\right) \quad . \quad . \quad . \quad . \quad . \quad (15).$$

Springs with a Rectangular Section.—For practical purposes it is obviously more convenient to use in the construction of our springs thin strips of a rectangular section rather than of an elliptic section. We have, therefore, now to consider how equations 13, 14, and 15 will be modified if our strip has a rectangular section, the longer side of the section being $2a$ and the shorter side $2b$. In this case

$$\mathrm{B}=\tfrac{4}{3}ab^3\mathrm{E},$$

and since, as was discovered by M . Cauchy in 1829, the torsion rigidity of a rectangle bears to the torsional rigidity of an inscribed ellipse the proportion of their moments of inertia about a line drawn through the common centre perpendicular to their plane, in the case when one principal dimension is several times the other, it follows that

$$\mathrm{A}=\mathrm{N}\pi\frac{a^3b^3}{a^2+b^2}\times\frac{\frac{4}{3}ab^3}{\frac{\pi}{4}ab^3},$$

or

$$\mathrm{A}=\tfrac{16}{3}\mathrm{N}\,\frac{a^3b^3}{a^2+b^2}.$$

Hence

$$\phi=\frac{l\mathrm{F}r\sin\alpha\cos\alpha}{4ab^3}\left(\frac{3}{4\mathrm{N}}\,\frac{a^2+b^2}{a^2}-\frac{3}{\mathrm{E}}\right),$$

$$d=\frac{l\mathrm{F}r^2}{4ab^3}\left(\frac{3}{4\mathrm{N}}\,\frac{a^2+b^2}{a^2}\cos^2\alpha+\frac{3}{\mathrm{E}}\sin^2\alpha\right);$$

or, as b^2 is insignificant in comparison with a^2,

$$\phi=\frac{3l\mathrm{F}r\sin\alpha\cos\alpha}{4ab^3}\left(\frac{1}{4\mathrm{N}}-\frac{1}{\mathrm{E}}\right) \quad . \quad . \quad . \quad . \quad . \quad (16),$$

$$d=\frac{3l\mathrm{F}r^2}{4ab^3}\left(\frac{\cos^2\alpha}{4\mathrm{N}}+\frac{\sin^2\alpha}{\mathrm{E}}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (17).$$

We will now calculate the value of f, the maximum stress at any point in the rectangular section.

The shear stress at any point, xy, in such a section of the prism which has received a twist, τ, per unit of length is, where the axes of x and y are parallel to the edges of the section, and the origin the centre of the section, z being at right angles to the section,

$$N\left(\tau x+\frac{d\gamma}{dy}\right) \text{ in the directions of } y \text{ and } z,$$

and

$$N\left(-\tau y+\frac{d\gamma}{dx}\right) \text{ in the directions of } z \text{ and } x,$$

where γ equals $-\tau xy+$ an expression which vanishes if the thickness of the strip is very small.

Hence since

$$\frac{d\gamma}{dy}=-\tau x,$$

and

$$\frac{d\gamma}{dx}=-\tau y,$$

it follows that the shear stress at a point xy is

$$0 \text{ in the direction of } y,$$

and

$$-2N\tau y \text{ in the direction of } x,$$

so that the whole shear stress at a point is $-2N\tau y$, and the shear stress, q, at the middle of the longer edge of the rectangular section, where we may suppose the shear stress is the greatest, is $2N\tau b$.

Now if C is the twisting couple, we know that

$$\tau=\frac{C}{A},$$

hence

$$\tau=\frac{3Fr\cos\alpha}{16Na^3b^3}(a^2+b^2),$$

and

$$q=\frac{3Fr\cos\alpha}{8a^3b^2}(a^2+b^2).$$

Also, since $Fr\sin\alpha$ is the bending couple, the greatest stress due to bending,

$$p=\tfrac{3}{4}\frac{Fr\sin\alpha}{ab^2}.$$

Hence if f is the greatest total stress anywhere in a section, it may be shown that

$$f=\tfrac{3}{8}\frac{Fr}{ab^2}\left\{\sin\alpha+\sqrt{1+\frac{b^2}{a^2}\cos^2\alpha\left(\frac{b^2}{a^2}+2\right)}\right\}.$$

Or, as a is very large in comparison with b,

$$f=\tfrac{3}{8}\,\frac{Fr}{ab^2}\,(1+\sin\alpha)\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad (18).$$

If, as is usual in the case of the springs we employ, the edges of the strip nearly touch one another in two consecutive coils, and the angle of the spiral is 45°, we have the area of the cylinder which the metal strip approximately covers, or $\frac{2\pi rl}{\sqrt{2}}$, equal to the area of the strip, or $2la$, so that

$$\frac{r}{a}=\frac{\sqrt{2}}{\pi},$$

and equations (16), (17), and (18) may be simplified thus—

$$\phi\propto 0{\cdot}17\,\frac{lF}{b^3}\left(\frac{1}{4N}-\frac{1}{E}\right)\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad (19).$$

$$d\propto 0{\cdot}17\,\frac{lFr}{b^3}\left(\frac{1}{4N}+\frac{1}{E}\right)\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad (20).$$

$$f\propto 0{\cdot}29\,\frac{F}{b^2}\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad (21).$$

We have preferred to say that ϕ, d, and f are respectively proportional rather than equal to the expressions on the right hand side, because when the strip is wide in comparison with the radius of the cylinder about which it is bent, the strip receives in addition to the bending and shear strains which we have considered, a lateral bending also, and the exact effect of this we have not yet fully investigated.

Use of Spring to determine $\frac{E}{N}$.—Before proceeding to a description of the various measuring instruments in which we have applied this new form of spring, we may mention one interesting application of it to enable us to determine readily the ratio of the modulus of rigidity, N, to Young's modulus of elasticity, E, for any material. It is well known that the celebrated conclusion of MM. Navier and Poisson from Boscovich's molecular theory requires that the ratio of E to N should be 2·5 for all solids. Professor Stokes showed that this conclusion was impossible if its authors supposed it to apply to jellies and to india-rubber, and that it was probably untrue in the case of metals; and Wertheim, Kirchhoff, Thomson, Everett, and others have experimentally shown its untruth in the case of brass, iron, copper, and glass. In pursuance of the present investigation it has struck us that this ratio may be most conveniently determined by the use of our springs from one experiment.

When the spring is made of round wire, so that a and b are equal to one another, we have from equation (11)

$$\frac{r\phi}{d} = \tan\alpha \frac{\frac{E}{N} - 2}{\frac{E}{N} + 2\tan^2\alpha};$$

let $\frac{r\phi}{d}$ be called $\cot\beta$, then it can be shown that

$$\frac{E}{N} = -2\tan\alpha\tan(\alpha+\beta),$$

so that if α is 45°

$$\frac{E}{N} = \frac{\tan\beta + 1}{\tan\beta - 1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22).$$

In order to measure $\tan\beta$ most conveniently, we may employ (fig. 6) a pair of cylindric scales, the distance apart of which,

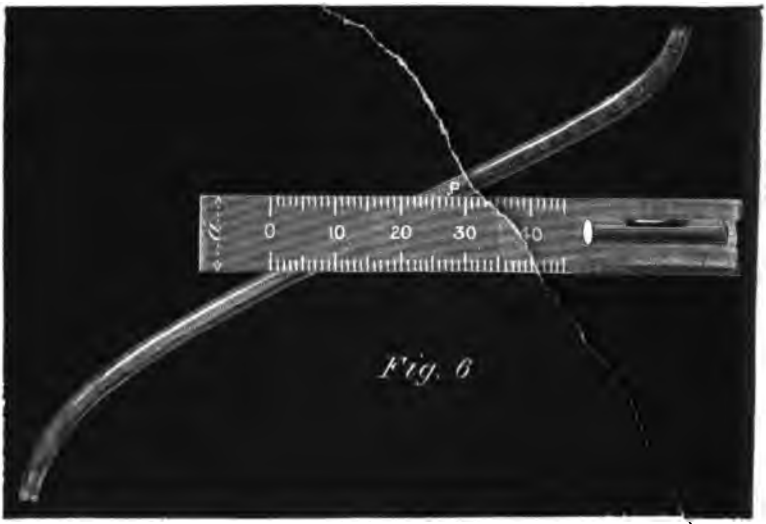


Fig. 6

close to the wire of the spring, is a inches. A point, P, on the wire is observed in its position in the upper scale, the reading being b. Now the spring is elongated by an axial force until the point, P, comes opposite a point on the lower scale, and the reading is now c. As the two scales are similar and parallel to one another, and as a spirit-level has been employed to make the scales horizontal, it is obvious that as the axis is vertical

$$\frac{a}{c-b}=\frac{d}{r\phi}=\tan\beta,$$

so that

$$\frac{\mathrm{E}}{\mathrm{N}}=2\frac{a+(c-b)}{a-(c-b)}.$$

We have used three springs made from round wire respectively of brass, iron, and steel for the sake of illustration. In all of them we find that $c-b$ is small in comparison with a, so that roughly we may say that the excess of $\frac{\mathrm{E}}{\mathrm{N}}$ over 2 is $\frac{4(c-b)}{a}$, that is, is proportional to $(c-b)$. The cylindric scale has only been introduced for the sake of illustration. For an accurate determination of $\frac{\mathrm{E}}{\mathrm{N}}$, a telescope would be employed having a motion about the axis of the spring, and also a motion parallel to the axis, and by means of which the motion of a point on centre line of the wire when an axial force is employed, would be accurately observed. It is sufficient to say that the brass spring of round wire exhibited to the Society gives a value of $\frac{\mathrm{E}}{\mathrm{N}}$ which is 8 per cent. less than the value given by the similar spring of round iron wire exhibited.

Instead of using a round wire, we may use for the experimental determination of $\frac{\mathrm{E}}{\mathrm{N}}$ a strip of rectangular section, whose breadth is very great in comparison with its thickness. If the angle of the spiral is 45°, then from equations (16) and (17) it follows that

$$\frac{\phi r}{d}=\frac{\frac{1}{4\mathrm{N}}-\frac{1}{\mathrm{E}}}{\frac{1}{4\mathrm{N}}+\frac{1}{\mathrm{E}}},$$

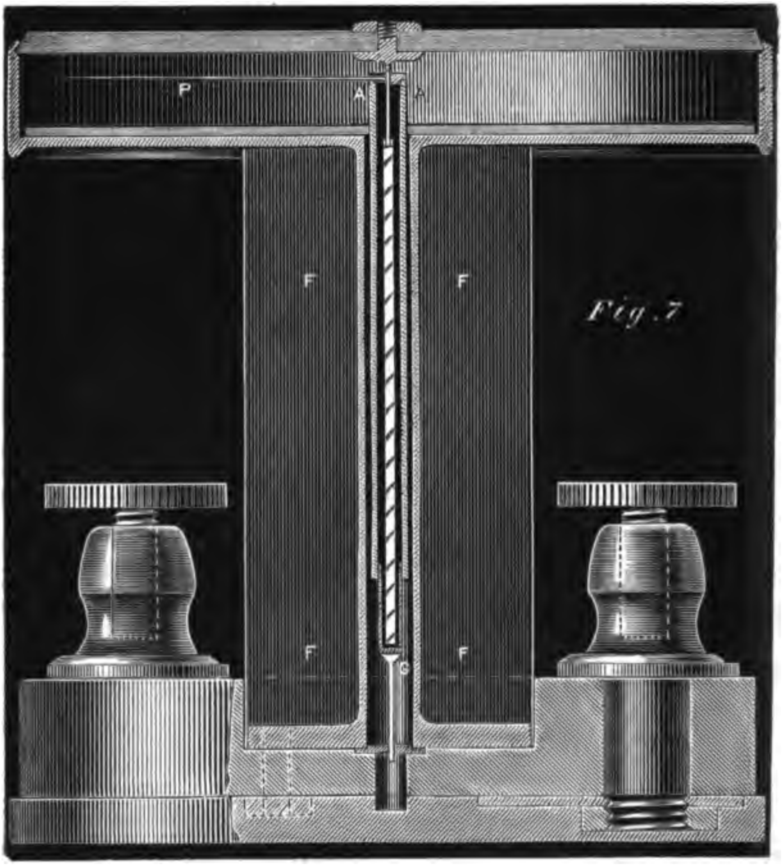
$$\frac{\mathrm{E}}{\mathrm{N}}=4\frac{d+\phi r}{d-\phi r};$$

therefore measuring ϕr as $c-b$, and d as a, by the method just described, we have

$$\frac{\mathrm{E}}{\mathrm{N}}=4\frac{a+(c-b)}{a-(c-b)}.$$

Some Practical Uses of the Springs.—By the employment of springs such as we have described, we have succeeded in making *ammeters* and *voltmeters*, or instruments for measuring respectively electric currents and differences of potential, in which the pointer moves over in some cases as much as 270° of the scale instead of only 50°, which is all that can be obtained with ordinary galvanometers. One form of the instrument is shown in fig. 7, where AA is a thin hollow tube

of charcoal iron attached at its lower end to a brass piece G guided at the bottom in the way shown. To G is attached the lower end of a spring made in the way we have described of silver or hard phosphor-bronze, the upper end of which is attached rigidly by a thin rod to the glass top of the instrument which itself is fastened rigidly



to the framework of the instrument. The rod attached to the glass, and to which the upper end of the spring is attached, also serves as a guide to the top of the iron tube. In the space FF a solenoid wire or strip is wound, its ends being attached to the terminals shown. Hence when a current is passed through the wire, the iron tube is sucked into the solenoid, and its lower end G, to which the spring is attached, receives a large rotatory motion, which is communicated directly to the pointer attached to the top of the iron tube. Parallax in taking

readings of the pointer is avoided by the horizontal scale being on looking glass in the well-known way.

By making the iron tube AA very thin, so that it is magnetically saturated for a comparatively weak current, by fixing it so that it projects into the solenoid a fixed distance which has been carefully determined by experiment, and by constructing the spring in conformity with the conditions worked out in this paper, so as to obtain a large rotation with minimum stress, and with not too much axial motion of the free end of the spring, we have succeeded in obtaining deflections up to 270° directly proportional to the current, and without any permanent set being given to the spring.

To prevent a spring taking a permanent set for a large deflection, it is of great importance that the spring after being delivered by the maker should receive a large degree of permanent set in the direction in which we wish it to be afterwards strained in ordinary working.

In spite of the fact that Professor J. Thomson in the "Cambridge and Dublin Math. Journ.," November, 1848, explained the importance of initial strains in materials, the reason is not yet sufficiently well understood why when a round bar has been well twisted beyond the limit of permanent set in a certain direction it has twice as much elastic strength to resist torsion in this direction as in the opposite direction. Now in the very act of manufacturing our springs, that is in the bending of the strip, the material acquires strains which are just opposite in character to the initial strains which we wish it to possess, for, as already explained, if the spring be constructed as in fig. 3, an extension of the spring produces a rotation tending to uncoil it. Hence a spring must not be regarded as ready for use until it receives a good set by means of a weight hung from its end.

Theory of the Solenoid Spring Ammeter or Voltmeter.—If C is the current in ampères flowing through the coil, the attractive force on the iron core is

$$\frac{KC^2}{1+SC},$$

where S is a constant, which is the greater as the current is smaller for which the iron tube AA, fig. 7, becomes saturated with magnetism. The position of this iron core in the solenoid is so selected that K remains practically constant throughout the small range of downward motion of the core.

Since the rotation ϕ has been produced by an axial force, we know from the theory of the spring already given, that this axial force is $p\phi$, where p is some constant. Hence

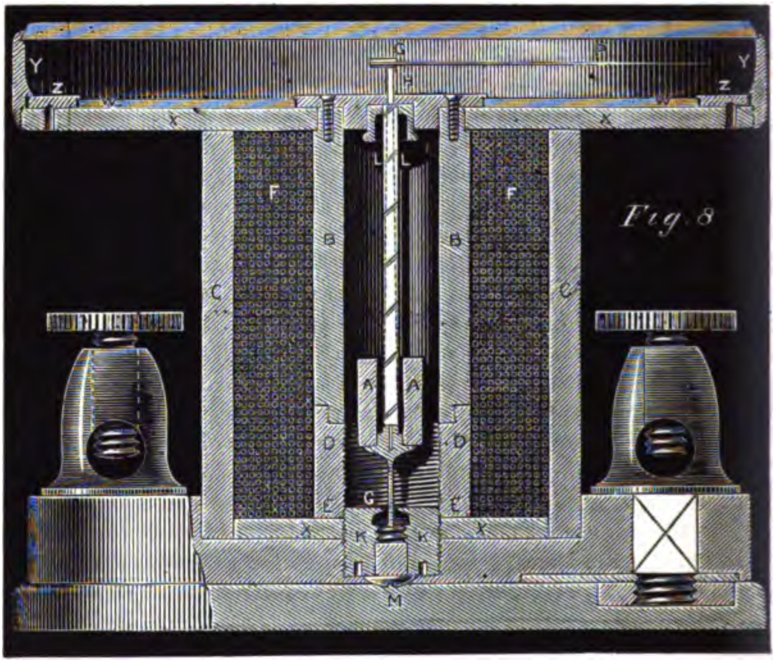
$$p\phi=\frac{KC^2}{1+SC},$$

and since SC is great in comparison with unity for such currents as we wish to measure, we have

$$\frac{p\phi}{K}=\frac{C}{S}-\frac{1}{S^2},$$

or

$$C=\frac{Sp}{K}\phi+\frac{1}{S},$$



that is, equal divisions of the scale correspond with equal additions to the strength of the current except close to the zero, and we usually do not graduate these instruments within 5° of the zero.

Shielded Measuring Instruments.—When it is desired to use the instrument close to a dynamo machine or electromotor in action, we have adopted a different and somewhat special form of construction, which is shown in fig. 8, by means of which the instrument is to a great extent shielded from even powerful external magnetic fields. In this instrument the electromagnet consists of a hollow core, part of which, BB, is of charcoal iron, and part, DE, of brass or other non-magnetic metal. The outside tube, CC, and the plates, XX, top and bottom, are also of charcoal iron. The space FF is filled with insulated wire in electrical connexion with the terminal, so that when a current is sent through the instrument an intense magnetic field is

formed between D and E, which are the poles of the electromagnet. To the iron tube AA, also made of charcoal iron, the spiral spring, in this case made of extremely thin hard steel, is attached, the other end being attached to the piece F, which is fixed relatively to the bobbin. The spindle GG, which is fixed to the moving iron core AA, moves freely in bearings HH, so that the only movements of which A is capable are one of rotation and one parallel to the axis of the bobbin. As the iron core A projects into the strong magnetic field between D and E it is strongly attracted towards E when the current flows, and, as before, causes a large rotation of the pointer P over the scale. As a means of varying the power of the instrument an adjustable iron piece K is provided, which can be screwed nearer to or farther from the core A, and by the use of which the sensibility of the instrument can be adjusted so as to make the instrument "direct reading," that is to say, each division of the scale can be made to correspond with 1 ampère of current, or 1 volt difference of potential, and the employment of a constant such as 1·34 ampères, or volts, per degree, which has hitherto been necessary with our measuring instruments, is now avoided. This power of adjustment produced by the use of the movable iron piece K, combined with the ease with which more or less wire can be wound on to the instrument, which also constitutes a second adjustment of sensibility, is of considerable importance, since the employment of a constant has not only led to error and delay in measurements made in electric light factories, but has caused the breakage of the pointer or the destruction of an instrument from a far too powerful current being sent through it by an observer (often a man with little experience in the employment of instruments) having confounded the constant of some other instrument with that of the one he was using.

The steel strip used in the construction of the spring for these shielded instruments is 0·0017 inch thick, and in a book just published on "Steel and Iron," by Mr. Greenwood, he mentions that some specimens of remarkably thin steel, $\frac{1}{1000}$ of an inch, were shown in the Paris Exhibition, so that steel we are using is nearly as thin as that referred to by Mr. Greenwood. But within the last few days we have received from Mr. Charles Jowitt, of Sheffield, a specimen of steel rolled for us only just over 0·001 inch in thickness, which is perhaps the thinnest steel yet made.

We have to thank one of our assistants, Mr. Bower, for so earnestly carrying out a long series of experiments on these very delicate springs.

Weighing Machines.—Another class of instruments in which we have practically employed this spring are weighing machines, and Fig. 9 shows one of the arrangements we adopt. The scale-pan is prevented from turning by the part AB being square and fitting very [illegible]ly a square hole in C. This arrangement introduces practically

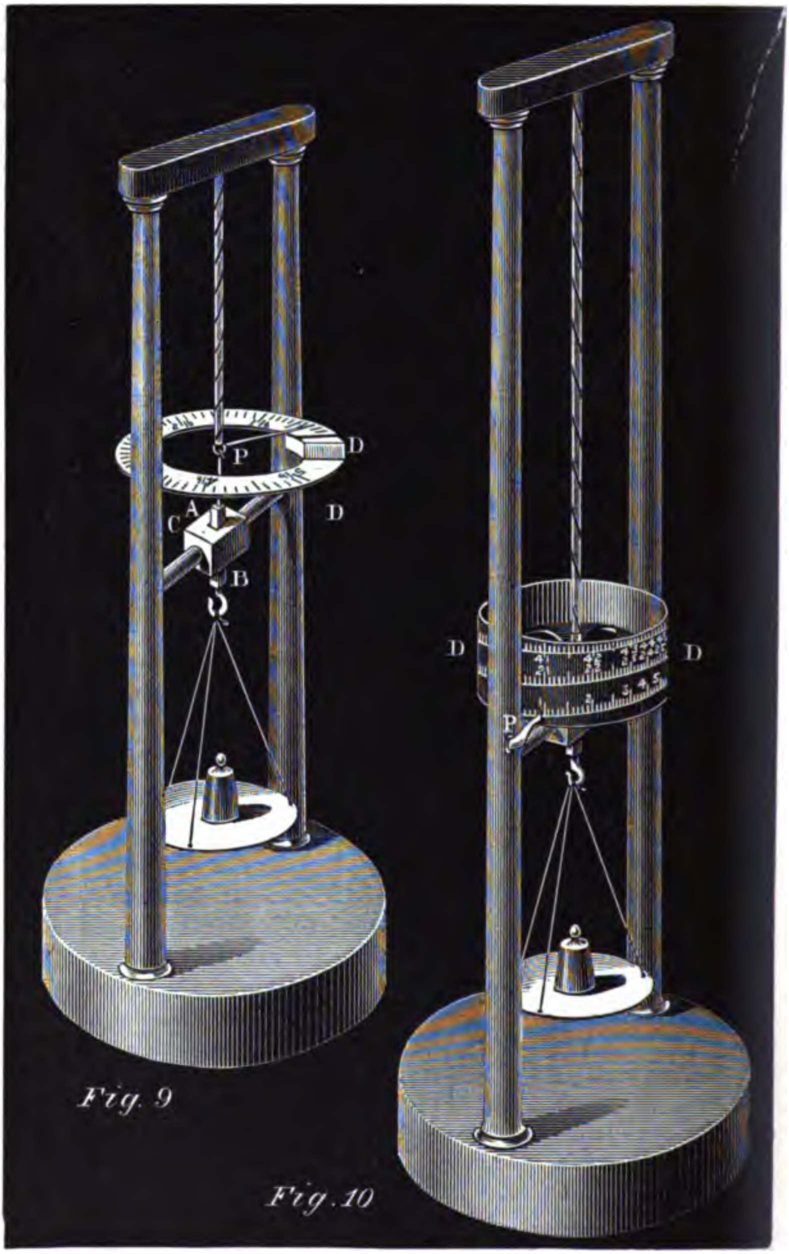


Fig. 9

Fig. 10

no friction, and prevents the moment of inertia of the scale-pan and load interfering, by means of a rotatory motion, with the rapidity with which the pointer comes to rest when a load is put into the pan. The position of the pointer P, which revolves when a weight is placed in the scale-pan, is read off upon the spiral scale, D, which, in this specimen, we have graduated in pounds. In another of these weighing machines, shown in fig. 10, the arrangement is the same with the exception that a cylindric scale D is fixed to the end of the spring and turns with it, the pointer P fixed on the frame of the instrument points to an indication of the weight on a spiral line drawn on the cylinder D. This second arrangement allows us to employ springs whose ends have a relative motion of five or six revolutions.

We have also made certain weighing machines where the weight is placed on a pan resting on the top of a rod passing up through the spring, and attached to the bottom of the spring, but hitherto we have found that this arrangement introduces too much friction.

When a very long spring is required for any purpose, and where the weight of the spring itself would cause greater stresses on the upper part than on the lower part of the spring, it is our custom to let the breadth and thickness of the strip of material remain unaltered, but to let the diameter of the coils diminish towards the upper parts. The formulæ already given suffice to show how this may be done so as to obtain uniformity of maximum stress at all sections.

We also present before the Society a model showing a combination of bifilar and spiral spring suspension, in which great rotation and small axial lengthening or shortening are produced by an axial force.

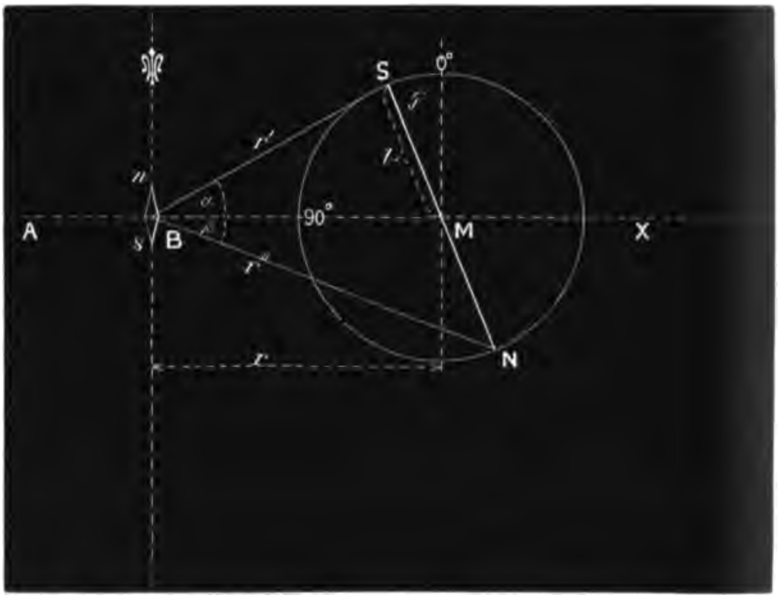
III. "Note on the Theory of the Magnetic Balance of Hughes." By Professor SILVANUS P. THOMPSON, B.A., D.Sc., Univ. Coll., Bristol. Communicated by Professor D. E. HUGHES, F.R.S. Received February 7, 1884.

[1.] The magnetic balance recently described by Professor D. E. Hughes* promises to be so convenient and useful an instrument in the laboratory, that the theory of its action and graduation deserves attention. In the actual instrument constructed by Hughes the graduation was empirically determined for a number of values, the remainder being found by interpolation. The instrument consists of a small suspended needle lying in the magnetic meridian provided with a zero-mark placed upon a platform in which a horizontal groove is cut magnetically east and west. In this groove the piece of iron

* "Proc. Roy. Soc.," vol. 36, p. 167.

or steel whose magnetism is to be tested is laid, in the "first position" of Gauss ("end on"), within its magnetising coil, a second coil being added on the other side of the suspended needle to compensate the action due to the coil alone. At a certain distance along the platform, and having its centre upon the line of the platform groove, is set a magnet—called by Hughes the "compensator"—of considerable magnetic moment. The compensator is so pivotted as to be capable of being rotated round a vertical axis through its centre over a scale; and, according to the original design of the instrument, the compensator and scale are provided with an arrangement whereby they may be shifted along the platform, so that they can be made to approach nearer to the suspended needle when a more powerful compensation is desired, or removed further away when a more delicate magnetic force is to be compensated. In practice the balance was obtained by fixing the central pivot of scale and compensator at a distance of 30 or more centims., and turning the compensator upon its pivot until its magnetic force on the suspended needle, or rather its resolved part in the axial line of the platform, was exactly equal and opposite to that of the piece of iron or steel.

FIG. 1.



[2.] *Theory of the Magnetic Balance.*—In fig. 1, let the line AX represent the central line of the platform of the balance lying magnetically east and west. The point M is the centre of the

compensator NS, which has been turned through an angle $OMS=\phi$ in order to balance the magnetic force exerted on the suspended needle at B by the specimen of iron or steel placed at A. It is required to determine the relation between the angle of turning ϕ, and the magnetic force thereby brought to bear along the axis AX at B tending to thrust back the suspended needle to its zero point. Let the length of the compensator be called $2l$, and the distance of B from $M=r$. It is evident that, in general, the resultant magnetic force at B due to the compensator will not be along the axis AX, but may be resolved into a part acting at right angles to that axis, and a part parallel to it. The former part will be parallel to the magnetic meridian, and, therefore, when the needle is at zero, this part will not tend to move the needle to either side. Its only action is to increase or diminish (according to circumstances) the directive force of the earth's magnetism upon the needle, and render it either more or less sensitive. The other part is that which acts along the axis and balances the magnetic force of the iron or steel bar at A. When the compensator lies at right angles to AX this component of the force vanishes by symmetry: consequently the zero of the scale lies in this line. If the compensator be turned through an angle ϕ, the component of its magnetic force in the line AX will increase to a maximum when $\phi=90°$. The values of the force for different angles may be calculated as follows:—

$$\text{Call} \quad BS=r',$$

$$BN=r'',$$

$$\text{angle } SBM=\alpha,$$

$$\text{angle } NBM=\beta.$$

Then the forces along BS and BN resolved along AB will give the following resultant F:—

$$F=m\left\{\frac{\cos\alpha}{r'^2}-\frac{\cos\beta}{r''^2}\right\};$$

m being the strength of the pole of the compensator.

But
$$\cos\alpha=\frac{r-l\sin\phi}{(r^2+l^2-2rl\sin\phi)^{\frac{1}{2}}};$$

$$\cos\beta=\frac{r+l\sin\phi}{(r^2+l^2+2rl\sin\phi)^{\frac{1}{2}}};$$

and
$$r'=(r^2+l^2-2rl\sin\phi)^{\frac{1}{2}};$$

$$r''=(r^2+l^2+2rl\sin\phi)^{\frac{1}{2}}.$$

Whence
$$F=m\left\{\frac{r-l\sin\phi}{(r^2+l^2-2rl\sin\phi)^{\frac{3}{2}}}-\frac{r+l\sin\phi}{(r^2+l^2+2rl\sin\phi)^{\frac{3}{2}}}\right\} \quad . \quad (1),$$

or, writing n for the ratio r/l,

$$F=\frac{m}{l^2}\left\{\frac{n-\sin\phi}{(n^2+1-2n\sin\phi)^{\frac{3}{2}}}-\frac{n+\sin\phi}{(n^2+1+2n\sin\phi)^{\frac{3}{2}}}\right\} \quad . \quad (2).$$

which for brevity we may write

$$F=\frac{m}{l^2}P$$

where
$$P=\frac{n-\sin\phi}{(n^2+1-2n\sin\phi)^{\frac{3}{2}}}-\frac{n+\sin\phi}{(n^2+1+2n\sin\phi)^{\frac{3}{2}}}$$

It is obvious from the mere form of this expression that it is not accurate to assume that a scale of equal degrees will represent equal increments of the magnetic force; and that no scale ordinarily used in galvanometers or magnetometers, such as a scale of sines or of tangents, will adequately serve the purpose. It is, therefore, necessary to investigate the formula to ascertain how it can be made available for the graduation of a scale for the instrument.

The expression is an awkward one for numerical calculation, owing to the fractional exponents of the denominators; but the process of calculation can be somewhat shortened by taking n as a simple integer, and the successive values of ϕ such that the sines are simple decimals. In the following tables will be found the values of the function P, calculated for several cases, the angles being in all cases expressed in degrees and decimals of degrees, not in degrees, minutes, and seconds. The object in calculating these values was to ascertain the proportions of r and l, which would enable the scale readings of the compensator to be used without the labour of constructing arbitrary interpolation tables for the calibration of the readings.

Table I.—(n=1.)

Angle.	Nat. sine.	P calculated.
0°	0	0
5·739	0·1	0·0355
11·538	0·2	0·0726
17·457	0·3	0·1195
23·578	0·4	0·1576
30·000	0·5	0·2113
36·869	0·6	0·2794
44·427	0·7	0·3741
53·130	0·8	0·5271
64·158	0·9	0·8618
71·805	0·95	1·3280
90·000	1·0	∞

Table II.—(n=2.)

Angle.	Nat. sine.	P calculated.
0°	0	0
10·000	0·1736	0·0444
20·000	0·3420	0·0938
30·000	0·5000	0·1568
40·000	0·6428	0·2235
50·000	0·7660	0·3369
60·000	0·8660	0·4787
70·000	0·9397	0·6534
75·000	0·9659	0·7412
80·000	0·9848	0·8168
85·000	0·9962	0·8736
90·900	1·0000	0·8888

Table III.—(n=3.)

Angle.	Nat. sine.	P calculated.
0°	0	0
5·739	0·1	0·0107
11·538	0·2	0·0219
17·457	0·3	0·03256
23·578	0·4	0·0462
30·000	0·5	0·0604
36·869	0·6	0·0764
44·427	0·7	0·0955
53·130	0·8	0·1188
64·158	0·9	0·1484
71·805	0·95	0·1664
90·000	1·0	0·1875

Table IV.—(n=4.)

Angle	Nat. sine.	P calculated.	P observed.
0°	0	0	0
5·739	0·1	0·005218	0·00556
11·538	0·2	0·010521	0·01136
17·457	0·3	0·010001	0·01716
23·578	0·4	0·021758	0·02271
30·000	0·5	0·027910	0·02833
36·869	0·6	0·034709	0·03488
44·427	0·7	0·041985	0·04198
53·130	0·8	0·050319	0·05136
58·211	0·85	0·054926	0·05605
64·158	0·9	0·059886	0·06061
71·805	0·95	0·065260	0·06554
90·000	1·0	0·071111	0·07290

Table V.—(n=5.)

Angle.	Nat. sine.	P calculated.	P observed.
0°	0	0	0
5·739	0·1	0·002848	0·00267
11·538	0·2	0·005809	0·00545
17·457	0·3	0·008772	0·00848
23·578	0·4	0·011715	0·01083
30·000	0·5	0·014895	0·01468
36·869	0·6	0·018257	0·01759
44·427	0·7	0·021829	0·02183
53·130	0·8	0·025741	0·02564
58·211	0·85	0·027819	0·02645
64·158	0·9	0·030000	0·02801
71·805	0·95	0·032297	0·03081
90·000	1·0	0·034722	0·03365

The values of P set down as observed in the last column of the preceding tables were obtained by noting the deflexions produced on a tangent magnetometer, with very small needles, by a magnet placed at the requisite distance (measured horizontally at right angles to the magnetic meridian), capable of rotation in a vertical plane passing through its centre and through that of the magnetometer. The readings were then multiplied by such a constant as would make the value at 44°·427 coincide with the calculated value at the same angle. The departures at other parts of the scale are partly due to errors of observation, partly due to the fact that the distance between the poles of the magnet is less than the distance between the ends.

Table VI.—(n=6.)

Angle.	Nat. sine.	P calculated.
0°	0	0
5·739	0·1	0·001708
11·538	0·2	0·003428
17·457	0·3	0·005163
23·578	0·4	0·006966
30·000	0·5	0·008814
36·869	0·6	0·010735
44·437	0·7	0·012749
53·130	0·8	0·014796
58·211	0·85	0·015975
64·158	0·9	0·017150
71·805	0·95	0·018348
90·000	1·0	0·019592

Table VII.—(n=10.)

Angle.	Nat. sine.	P calculated.
0°	0	0
5·739	0·1	0·000390
11·538	0·2	0·000778
17·457	0·3	0·001169
23·578	0·4	0·001505
30·000	0·5	0·001966
36·869	0·6	0·002374
44·427	0·7	0·002784
53·130	0·8	0·003207
64·158	0·9	0·003638
71·805	0·95	0·003858
90·000	1·0	0·004090

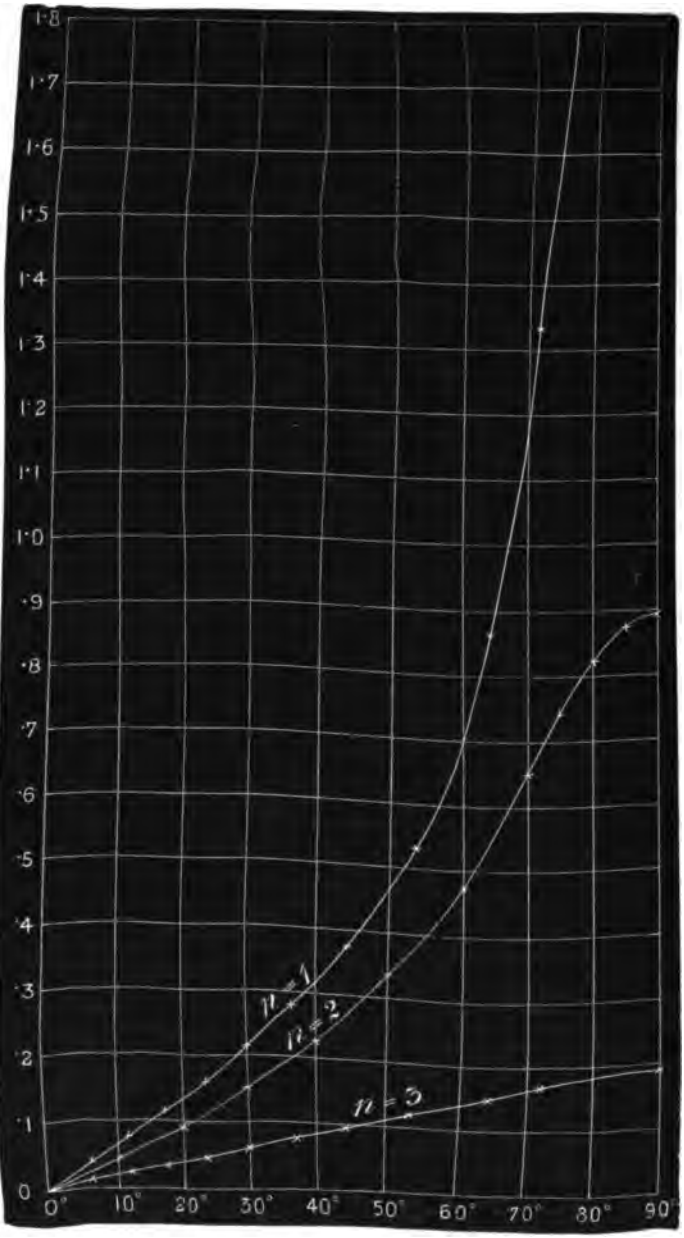
The calculated values of P are plotted out in the two sheets of curves (figs. 2 and 3) accompanying the tables. The curves for n=4 and n=5 are given twice, being drawn again with enlarged ordinates to show on a larger scale their close approximation to straight lines up to about 50°.

[3.] From the foregoing tables, and from the curves appended, several conclusions may be immediately drawn with respect to the design of the magnetic balance. A simple inspection of the curves will show that they belong to a family in which, in general, there is a concavity near the origin, and a convexity as the limiting value is approached at the point corresponding to the 90° position of the compensator. Those curves for which n=3, or less than 3, show the convexity very markedly. In those for values of n higher than 7 (only one has been drawn, namely, that for n=10) the convexity of the upper portion asserts itself. The curve for n=10 approximates very nearly to a curve of sines, as indeed might be suspected from the equation.

For those values of n which lie between 4 and 6 inclusive, the first half of the curve is very nearly a straight line, so nearly so that for the curves n=4 and n=5 the values of the ordinates do not differ by 1 cent. from those which they would have for actual straight lines lying along the mean slope of the respective curves as far as 45°. In other words, for angles less than 45° *the magnetic force* exercised along the axis of the balance by the compensator when it is turned *is proportional*, within 1 per cent. of accuracy, *to the angle through which it has been turned, provided the distance of the compensator from the needle be not less than four and not greater than five times the half-length of the compensator.*

This result may be verified from equation (2) by finding what

Fig. 2.


1·8
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0
0°
10°
20°
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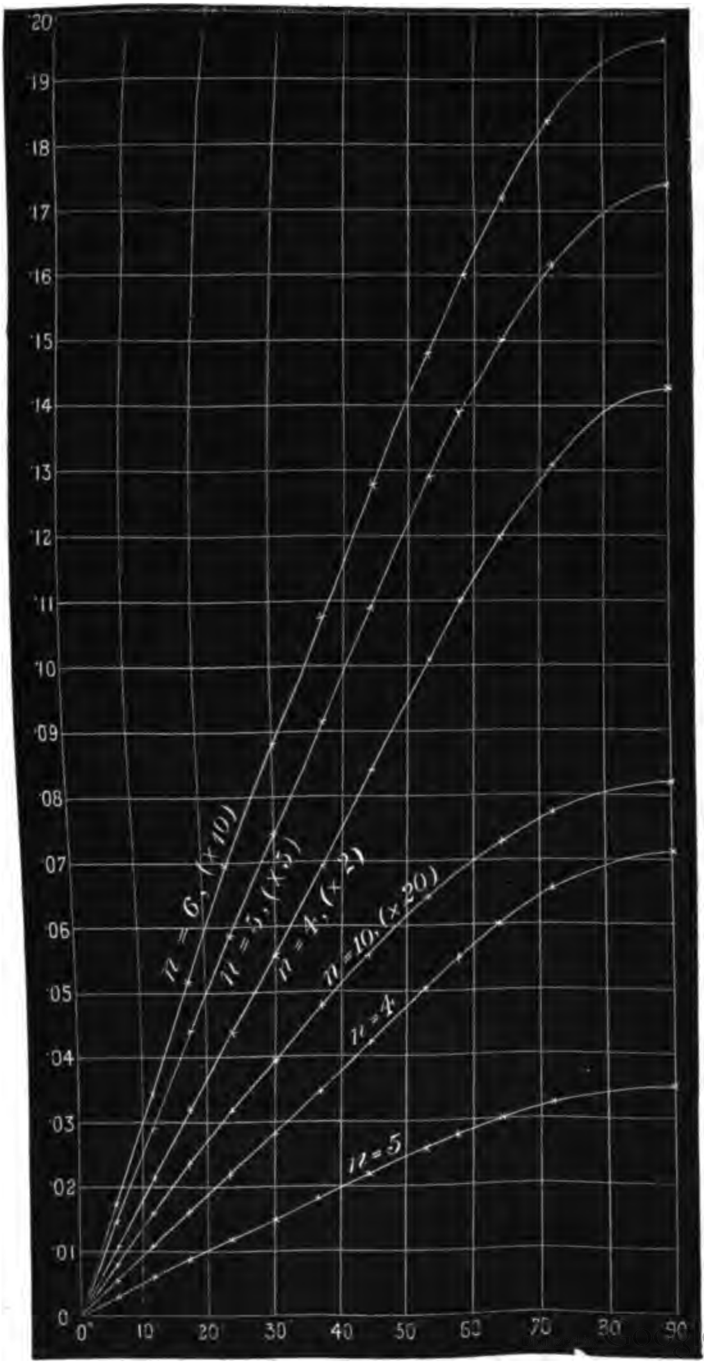
40°
50°
60°
70°
80°
90°
n = 1
n = 2
n = 3

Fig. 3.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·01
0
0°
10
20
30
40
50
60
70
80
90
n = 6, (×10)
n = 5, (×5)
n = 4, (×2)
n = 10, (×20)
n = 4
n = 5

value of n will give the force when $\phi=45°$ equal to $\frac{3}{2}$ of the force when $\phi=30°$. Writing in the sines of these angles and equating 3 times the force at 30° to 2 times the force at 45°, we get the value $n=4{\cdot}6$. Or, solving similarly for the case that the force at 60° shall be double the force at 30°, we get $n=4{\cdot}28$. Here, however, the errors near the intermediate angle of 45° are not inconsiderable, exceeding 4 per cent. In any case we shall not be far wrong if we adjust the balance so that $n=4{\cdot}5$, and confine our readings within 45° of zero. For purposes where great accuracy is not essential the readings might be extended as far as 60°, since the curves for $n=4$ and $n=5$, show how very little the straight line is departed from up to that point. In the original balance of Hughes, the values of r and l are about 30 centims. and 7·5 centims. respectively, so that $n=4$.

[4.] I now proceed to the consideration of the action of the compensator in affecting in different positions the sensitiveness of the needle.

In the original instrument of Hughes, the compensator turned in a horizontal plane; and therefore in every position, save only when at 90° to the zero of its scale or when end-on to the indicating needle, the force exercised by it on the needle would have a component in the magnetic meridian. As remarked above, the effect of this component would be to increase or diminish the directive force of the earth's magnetism upon the needle, according as the S pole of the compensator pointed northwards, or southwards. The sensitiveness of the indicating needle will, of course, be affected by this component; being a maximum when the component is such as nearly to astatise it. But it is evident that if the needle be nearly astatised when the compensator is at zero, it will not be so astatised when the compensator is moved right or left. The sensibility of the needle will diminish as the effective force of the compensator increases by its being turned. The calculation for the component of the force at this point is as follows:—

Calling T the part of the magnetic force at B (fig. 1) resolved at right angles to AX, we have—

$$\mathrm{T}=m\left\{\frac{\sin\alpha}{r'^2}-\frac{\sin\beta}{r''^2}\right\}.$$

But
$$\sin\alpha=\frac{l\cos\phi}{(r^2+l^2-2rl\sin\phi)^{\frac{1}{2}}},$$

and
$$\sin\beta=\frac{l\cos\phi}{(r^2+l^2+2rl\sin\phi)^{\frac{1}{2}}},$$

whence
$$\mathrm{T}=ml\cos\phi\left\{\frac{1}{(r^2+l^2-2rl\sin\phi)^{\frac{3}{2}}}+\frac{1}{(r^2+l^2+2rl\sin\phi)^{\frac{3}{2}}}\right\}.$$

Writing, as before, $r=nl$, this becomes:—

$$T=\frac{m}{l^2}\cos\phi\left\{\frac{1}{(n^2+1-2n\sin\phi)^{\frac{3}{2}}}+\frac{1}{(n^2+1+2n\sin\phi)^{\frac{3}{2}}}\right\} \quad . \quad (8).$$

Assuming that the balance is adjusted for the case $n=5$, we get the following values for this complex function of ϕ:—

$$\phi=0, \qquad T=\frac{m}{l^2}\times\cdot01508.$$

$$\phi=30^\circ, \qquad T=\frac{m}{l^2}\times\cdot01402.$$

$$\phi=60^\circ, \qquad T=\frac{m}{l^2}\times\cdot00938.$$

$$\phi=90^\circ, \qquad T=\frac{m}{l^2}\times0.$$

The needle, therefore, if nearly astatised when $\phi=0$, will still be nearly astatised when ϕ does not exceed 30°; but beyond this value the meridional component of the force falls off greatly, and the sensitiveness alters correspondingly.

[5.] *Suggested Modifications in the Magnetic Balance.*—The preceding investigation has led to the following suggestions:—

1stly. The compensator should be set so as to revolve against a vertical circle placed at right angles to the magnetic meridian, having its centre on the same level as the indicating needle, and magnetically east or west of it. In this position, which was that chosen for the experiments in which the values of P were observed for the purpose of comparison with the calculated values in the preceding part of these notes, the magnetic force of the compensator has no resolved part in the magnetic meridian at the point where the indicating needle is placed. The sensitiveness of the needle is, therefore, the same in all positions to which the compensator may be turned in its own plane.

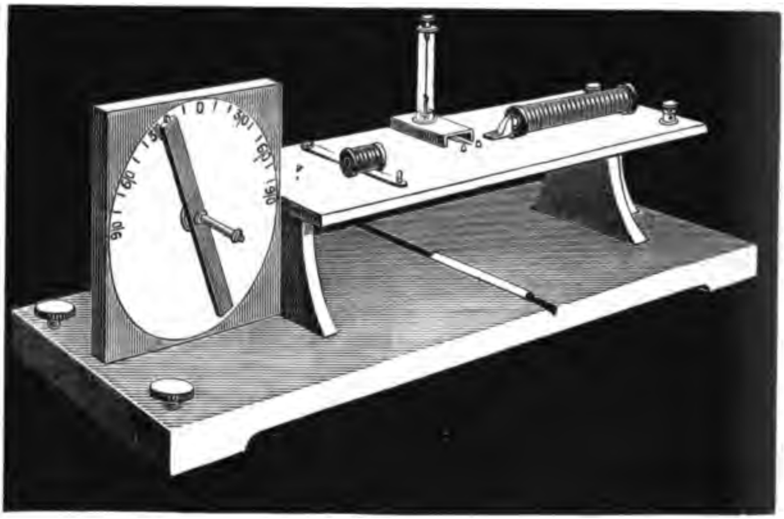
2ndly. A small magnet lying magnetically north and south of the indicating needle is used to astatise it to the required degree.

3rdly. The compensator is placed so that the distance between its centre and that of the indicating needle is about 4·6 times the half-length between the poles of the compensator. If the latter be a not very thick flat straight magnet, it may be approximately assumed that the poles are about one-tenth of the whole length from the ends, so that no very important error can arise if a distance equal to five times the half-length between the ends of the magnet is selected. When this is done it is safe to assume that for all positions of the compensator within 55° on either side of zero, the magnetic force which it exerts on the pole of indicating needle is proportional within 2 per cent. to the degrees of the scale through which the compensator

has been turned; and is proportional within 1 per cent. for angles less than 45°. For the practical purposes for which the magnetic balance is designed there will therefore be no need of a special calibration table.

4thly. As it is inadmissible to obtain a great range of compensation for both large and small magnetic forces by diminishing or increasing the distance of the compensator from the indicating needle, it is proposed to obtain this range by placing over the compensator a second magnet, capable of rotating round the same axis and having the same length between its poles. A third magnet may be added, similarly, to increase the magnetic moment of the compensator.

Fig. 4.



5thly. In the preceding investigation it has been assumed throughout that the length of the indicating needle was small relatively to r and l, so that the terms of the second and higher orders due to its length might be neglected. This was not the case in the original instrument of Hughes, in which the needle was 5 centims. in length. The use of a very small needle is open to the objection that it is not so sensitive as an index. It is therefore proposed to substitute as an indicating needle one of the type sometimes termed "unipolar," that is to say, having one pole set in the axis of rotation, so that only one pole has a moment of couple about the axis of suspension. A steel needle about 5 centims. long has about 1·5 centims. of its length turned up at right angles, the suspending fibre of silk being attached to the turned-up end; a counterpoise is added behind, and a small additional weight is placed below on a brass wire attached immediately

below the centre of suspension. This needle is placed, with its one effective pole in the axial line of the balance, level with the centre of the compensator.

The balance, with the modifications described, is represented in fig. 4.

February 21, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On some Relations of Chemical Corrosion to Voltaic Current." By G. GORE, F.R.S., LL.D. Received February 11, 1884.

The chief object of this research was to ascertain the amounts of voltaic current produced by the chemical corrosion of known weights of various metals in different liquids. The research was also made to throw light upon the conditions which determine the entire conversion of potential molecular energy into external (*i.e.*, available) electric current. The method adopted was to take about six or eight ounces by measure of a corrosive electrolyte and divide it into two equal volumes in two similar glass vessels. Two pieces of metal of equal dimensions were then cut from the same sheet, cleaned perfectly, and weighed. One of the pieces was employed as the positive plate of a voltaic cell in one of the portions of liquid, and the other as a comparison corrosion sheet in the other portion. The negative metal of the voltaic cell was in nearly all cases a large cylinder of sheet platinum, and surrounded the positive one. The positive and comparison plates were wholly immersed in the separate portions of liquid, except that the former had a narrow exterior projecting strip for connexion in a circuit. The current from the cell was passed by means of a small sheet silver anode ($\frac{1}{2}$ inch by $\frac{3}{4}$ inch), and a smaller silver cathode ($\frac{1}{4}$ inch by $\frac{3}{8}$ inch) in a third vessel, through a cyanide of silver plating solution containing the least practicable proportion of free potassic cyanide in order to obtain the maximum amount of silver deposit.

During each experiment observations were made of any liberation

of gas or formation of solid coating upon the plates, and of any other circumstance which appeared likely to affect the speed of corrosion, or weight of the plates, or of the deposited silver; and in any case where any solid coating was found, it was entirely removed after the experiment, and previous to ascertaining the losses of weight by corrosion. After the experiment the plates were well washed and carefully dried between hot sheets of filter-paper before weighing. The metals and liquids employed were as pure as could conveniently be obtained, and distilled water was used for all the solutions. The sample of potassic cyanide usually employed was found by analysis to contain 89·14 per cent. of the actual substance. Nearly all the liquids were at the atmospheric temperature, and in nearly every case the comparison sheet was employed.

As the proportion of silver deposited to the amount of external current generated, and therefore also to the amount of positive metal corroded, varied to a very small extent with the density of the current at the cathode in the depositing solution (see "Electrolytic Balance of Chemical Corrosion," "Proc. Birm. Phil. Soc.," vol. iii, 1883, pp. 278–304), the amount of cathode surface was usually varied according to the apparent strength of the current and rate of deposition, being smaller as the strength of current was less.

The degrees of electromotive force were measured by the aid of two thermoelectric batteries, each consisting of about 300 pairs of iron and German silver, together with a Clark's Standard cell (see "Proc. Birm. Phil. Soc.," vol. iv, Pt. I).

The following table exhibits the results. In the table the number of square inches given includes in each case the immersed portion of both surfaces of the positive metal and comparison plate. The losses of the plates, and the weight of deposited silver, are in every instance expressed in grains, and the rates of loss of the positive plate are given in column 6 in grains per square inch per hour. If the whole of the corroded portion of the positive metal produced external current, consistent in amount with the atomic weights and valencies of the metals as given in the table, and if that amount be called 100, then the proportions of such current actually obtained in the experiments are those given in column No. 9. The quantities of substance mentioned in column No. 1 were in each case per ounce of water.

Remarks.

Only those results which appeared reliable are inserted in the table; many uncertain ones were omitted. In some cases coatings of solid matter formed either upon the positive or comparison plates, or gas was evolved by the plates. Cases in which the coatings could not be perfectly removed were rejected. With magnesium in solutions of ammonic, potassic, or sodic chloride, potassic bromide or iodide, a

No. of Experiment.	1. Voltaic elements.	2. Square inches.	3. Hours.	4. EMF. in volts.	5. Loss by positive plate.	6. Rate of loss.	7. Loss by comparison plate.	8. Weight of silver deposited.	9. Per cent. of equivalent of current.
	Mg″ 24·3.								
1	10 grains KCy*	1·5	3	1·095	·028	·006	·013	·220	88·42
2	10* „ „	3	5	„	·042	·002	·006	·355	95·10
3	„ „	1·5	17	·9046	·010	·0004	·003	·062	69·75
4	1¼ minims HNO_3	„	2	1·933	·577	·192	·560	·384	7·45
5	„ HCl	„	1	2·000	·378	·252	·378	·183	5·45
6	⅛ „ H_2SO_4	„	1·5	2·216	·081	·036	·109	·298	41·10
7	1¼ „ „	„	$\frac{1}{12}$	variable.	·168	1·344	·183	·070	4·68
8	10 „ $HFSiF_4$	„	·5	2·111	·257	·343	·116	·910	39·83
	Żn″ 65.								
9	100 grains KHO	4	4	1·2430	1·620	·101	·563	·909	16·88
10	10 „ KCy	3	1·5	1·085	·188	·042	·051	·484	77·45
11	100 „ „	4	5	1·1435	1·410	·0705	·391	2·996	64·00
12	1¼ minims HNO_3	3	·5	1·2726	·461	·307	·343	·137	6·45
13	5 „ HCl	„	1	1·185	1·142	·381	·482	·790	20·81
14	30 „ „	4	·5	1·225	1·334	·667	2·379	·354	8·10
15	1¼ „ H_2SO_4	3	„	1·117	·389	·259	·113	·283	21·90
16	10 „ $HFSiF_4$	4	2	1·156	·399	·050	·250	·639	48·70
	Amalgamated Zn.								
17	10 grains KCy	3	„	1·121	·360	·060	·077	·930	77·70
18	100 „	„	3	1·236	·599	·066	·082	1·418	71·25
19	5 minims HCl	„	4	1·220	·887	·074	·038	2·670	90·55

* This sample of cyanide was found by analysis to contain 81·7 per cent. of actual cyanide.

No. of Experiment.	1. Voltaic elements.	2. Square inches.	3. Hours.	4. EMF. in volts.	5. Loss by positive plate.	6. Rate of loss.	7. Loss by comparison plate.	8. Weight of silver deposited.	9. Per cent. of equivalent of current.
20	1¼ minims H_2SO_4	3	3	1·111	·609	·067	·060	1·658	81·90
21	1¼ " "	"	1	"	·184	·061	·001	·513	83·90
22	20 " "	"	2	1·2536	·856	·143	·049	2·599	91·35
23	100 " "	"	3	1·4407	1·166	·129	·076	3·488	90·02
24	100 " " *	"	6	·5098	·504	·028	"	1·175	72·84
	Cd″ 112.								
25	10 grains KCy	3	16	·8019	1·759	·036	·116	2·982	84·97
26	100 " "	4	7	·987	·720	·023	·021	1·264	91·00
27	1¼ minims HNO_3	3	1	1·0426	1·250	·417	·808	·393	16·30
28	5 " "	"	·5	·9622	2·129	1·42	5·090	·145	3·53
29	5 " HCl	"	4	·948	·541	·045	·041	·803	77·00
30	50 " "	"	3	1·017	1·040	·115	·061	1·783	88·85
31	1¼ " H_2SO_4	"	2	·862	·576	·096	·123	·839	75·50
32	50 " "	"	3	·9774	·814	·090	·323	·988	63·00
	Amalgamated Cd.								
33	10 grains KCy	"	15	·739	2·118	·047	·153	3·583	87·45
34	100 " "	"	6	1·033	·769	·043	·046	1·378	92·90
35	50 minims HCl	"	5	1·058	1·048	·069	·054	1·846	91·40
36	50 " "	$\frac{1}{16}$	"	"	·434	1·38	·004	·783	93·69
37	1¼ " H_2SO_4	3	4	·7463	·519	·043	·085	1·857	85·60
	Sn″ 118.								
38	100 grains KHO	5	6	·9552	·502	·0197	·059	·942	86·90

* The negative plate was in this case a large cylinder of sheet lead.

No. of Experiment.	1. Voltaic elements.	2. Square inches.	3. Hours.	4. EMF. in volts.	5. Loss by positive plate.	6. Rate of loss.	7. Loss by comparison plate.	8. Weight of silver deposited.	9. Per cent. of equivalent of current.
39	1¼ minims HNO_3	4	5	·8333	·882	·0441	·733	·036	2·23
40	Solution of 3·5 per cent. pure HF	5	„	·6872	·420	·017	..	·545	70·77
41	„ 14·0 „ „	„	4	·7411	·383	·019	..	·551	78·59
42	20 minims HCl	„	5	·7514	·205	·008	·054	·219	58·35
43	40 „ „	„	7	·7514	·323	·009	·070	·433	73·42
44	50 „ „	4	„	·8627	·375	·013	·077	·654	95·25
45	60 „ „	5	„	·8330	·370	·0106	·064	·533	78·70
46	·5 ounce „	„	4	·863	·419	·0209	·111	·493	64·25
47	10 grains chromic chloride	„	„	·6354	·105	·005	·062	·119	61·90
48	16 minims H_2SO_4	4	7	·756	·318	·0113	·061	·398	68·35
49	16 „ „	„	15	„	·334	·0055	·098	·228	37·30
	Amalgamated Sn.								
50	100 grains KHO	2	„	·9847	1·313	·0437	·127	1·933	80·45
51	1¼ minims HNO_3	„	2	·8689	·061	·015	·036	·086	77·01
52	50 „ HCl	„	6	·8085	·278	·023	·066	·328	64·45
	Pb'''' 207.								
53	5 grains KHO	4	6	·5797	·950	·039	·244	·781	39·39
54	50 „ „	„	„	·6196	·980	·041	·179	1·807	88·33
55	100 „ „	„	„	·6524	·843	·035	..	·722	41·04
56	10 „ KCy	„	5	·3037	·046	·0023	·027	·069	71·85
57	100 „ „	„	„	·2879	·245	·0122	..	·063	12·32
58	5 minims HNO_3	„	„	·9788	1·640	·082	1·247	·228	6·65
59	50 „ „	„	2	·9816	11·966	1·496	10·694	1·430	5·72
60	5 „ HCl	„	6	·701	·469	·019	·423	·196	20·25
61	1 ounce strong acetic acid	„	8	·5771	·277	·023	·155	·127	21·95

No. of Experiment.	1. Voltaic elements.	2. Square inches.	3. Hours.	4. EMF. in volts.	5. Loss by positive plate.	6. Rate of loss.	7. Loss by comparison plate.	8. Weight of silver deposited.	9. Per cent. of equivalent of current.
	Amalgamated Pb.								
62	50 grains KHO	4	6	·6244	1·181	·049	·135	1·048	42·52
63	1 ounce strong acetic acid	,,	4	·6444	·331	·0207	·122	·197	28·50
	Al''' 27·5.								
64	4 grains KHO	5	1	1·5117	·499	·083	·492	·499	8·43
65	10 ,, KCy	6	3	1·2146	·118	·006	·333	·924	66·50
66	Solution of 3·5 per cent. pure HF	5	·25	·9306	1·073	·858	1·003	·219	1·73
67	50 minims HCl	,,	·5	1·048	1·079	·43	1·254	·189	1·50
68	1¼ ,, H_2SO_4	,,	8	·594	·036	·0009	·022	·262	65·10
69	16 ,, ,,	,,	7	·736	·074	·002	·036	·556	71·66
70	16 ,, ,,	,,	17	,,	·127	·0015	·039	1·074	72·08
71	48 ,, ,,	,,	5	·6938	·051	·002	·020	·394	65·56
	Fe'''' 56.								
72	1¼ minims HCl	2	3	·687	·099	·016	·056	·169	22·10
73	5 ,, ,,	,,	4	·918	·252	·031	·071	·650	33·42
74	1¼ ,, H_2SO_4	,,	2	·7473	·142	·035	·051	·256	26·70
	Ni'''' 59.								
75	50 grains KCy	3	8	·4001	·293	·009	·080	·791	36·82
76	160 minims HCl	3	,,	·449	·106	·004	·056	·118	14·56
77	1¼ ,, H_2SO_4	,,	15	·2311	·147	·008	·038	·274	25·45
	Cu'' 63·5.								
78	10 grains KCy	6	1	·9706	·240	·040	·172	·123	15·05

No. of Experiment.	1. Voltaic.	2. Square inches.	3. Hours.	4. EMF. in volts.	5. Loss by positive plate.	6. Rate of loss.	7. Loss by comparison plate.	8. Weight of silver deposited.	9. Per cent. of equivalent of current.
79	100 grains KCy	6	1	1·1758	·530	·088	·190	·577	32·00
80	25 minims HNO_3	„	21	·4946	2·550	·020	·763	5·420	62·45
81	50 „ „	„	3	·6429	·746	·041	·899	1·858	73·20
82	48 „ HCl	4	19	·466	·510	·007	·181	·463	26·65
83	160 „ „	„	18	·3982	·384	·005	·125	·320	24·50
84	160 „ $HClO_3$	6	4	·6872	1·416	·059	1·109	·449	9·30
85	1¼ „ H_2SO_4	„	17	·3111	·170	·0016	·109	·079	18·65
86	48 „ „	„	19	·2947	·150	·001	·133	·156	21·30
87	Concentrated „	3	7	·4729	·075	·004	·017	·047	20·61
	Amalgamated Cu.								
88	10 grains KCy	6	3	·7368	·422	·023	·140	·502	34·95
	Ag 108.								
89	10 grains KCy	5	3	·4483	·219	·015	·057	·149	68·00
90	50* „ „	5·5	7	·570	1·021	·026	·274	·441	43·19
91	50 minims HNO_3	„	6	·4744	·815	·025	·011	·421	51·60
92	Concentrated HCl	„	8	·5639	·120	·003	·038	·054	45·00
93	1 ounce $HClO_3$	„	7	·1842	·228	·006	·000	·068	38·60
94	Concentrated H_2SO_4	„	3	·5007	·101	·006	·025	·073	78·20
95	Weak solution of ferric sulphate	„	5	..	3·230	·116	3·026	·043	1·30
	Amalgamated Ag.								
96	10 grains KCy	5·75	4	·4146	·240	·0104	·129	·116	48·00
97	50 minims HNO_3	„	6	·4357	1·516	·046	·000	·927	61·10

* This sample of cyanide was found by analysis to contain 81·7 per cent. of actual cyanide.

solid deposit containing black suboxide of magnesium formed upon the positive plate only.* And with aluminium in solution of iodic acid a compound of iodine was absorbed, and both the plates increased about 16 per cent. in weight.† In seven cases (Nos. 6, 7, 14, 28, 65, 67, 81), from causes which were not very apparent, the comparison sheets lost weight more than the positive ones. The considerable degree of variability of rate of ordinary corrosion detracted greatly from the usefulness of the comparison sheet.

The results show that the proportion of loss of the positive plate by "local action" to that by corrosion attending external current varied greatly in different cases; with silver in a dilute solution of ferric sulphate, the latter amounted to only about 1·3 per cent. of the total corrosion, whilst with tin in weak hydrochloric acid it was as much as 95·25 per cent. In no instance was the whole of the metal dissolved by "local action," and in no case was the whole of the corrosion attended by external current.

The amount of corrosion of the positive plate was in nearly all cases greater than that of the comparison one. Six cases were selected from the table, in which the apparent corrosion of the latter sheet was the greatest, and some trouble was taken to ascertain by experiment whether either of these was a real one. After varying the conditions of the trial in several ways, with pure copper in a mixture of 50 minims of pure nitric acid per ounce of distilled water, it was found that in this instance the comparison plate was always corroded the fastest.

As the amount of corrosion of the positive plate was in nearly all cases greater than that of the comparison one, and the proportion of gas to corrosion was also frequently less with the former than with the latter, a solid deposit was usually more readily formed on the positive plate than upon the comparison one, and the chemical products were not always exactly the same upon the two plates. An instance of this kind occurred with amalgamated silver in a solution of potassic cyanide; the positive plate alone became brown. As the chemical products of "local action," and of corrosion attended by external current, are usually the same, in cases where they are different, though resulting from the same materials, the total expenditure of potential molecular motion would probably be different, because the quantities of residual molecular energy of the ingredients would not be the same.

The proportion of corrosion of the positive plate accompanying external current, to that produced by "local action," may be approximately arrived at, either by deducting from the total amount of corrosion of that plate the loss by weight of the comparison one, or by

* See "Proc. Birm. Phil. Soc.," vol. iv.

† *Ibid.*, vol. iv, Part I, p. 65.

calculating from the quantity of deposited silver, together with the valencies and atomic weights of the positive metal and of silver, the amount of the positive metal required to produce the current. Each of these methods however is imperfect; the second is usually much the least so. In several cases, by employing the former one, the amount of deposited metal was found to be nearly equivalent to the amount of corrosion accompanying external current, after making allowance for a small deficiency of deposited silver caused by a portion of the current traversing the free potassic cyanide in the depositing liquid.

That the external current affects in some cases the amount of "local action" was shown by the following experiment:—A sheet of platinum and one of ordinary zinc, not connected together, were partly immersed in dilute sulphuric acid, until a moderate stream of gas arose from the zinc. On now connecting the two plates together by means of a wire, the stream from the zinc was conspicuously diminished. The contact of a negative metal with the positive one usually increased the total loss by corrosion, whilst it commonly decreased that caused by "local action."

The results contained in the table further show that the proportion of corrosion attending external current (as measured by the amount of silver deposited) to that by "local action," depends upon several conditions:—1st, upon the kind of metal; thus it was usually greater with cadmium (average 75·63 per cent.) than with copper (30·33 per cent.). 2nd, upon the kind of liquid; it was greater with solution of potassic cyanide (average 63·27 per cent.) than with dilute nitric acid (31·14 per cent.). And 3rd, upon the degree of concentration of the liquid; a greater strength of solution frequently increased, and as frequently decreased, the proportion of corrosion accompanying external current to that by "local action." A variation of kind of metal also was attended by a greater change in the proportion of such corrosions than a variation of kind of liquid. This latter circumstance is in harmony with the view that the potential molecular activity of metals is usually greater than that of electrolytes (see "Relations of Heat to Voltaic and Thermo-Electric Action," "Proc. Roy. Soc.," vol. 36, p. 50).

The effect of amalgamation of the plates upon the proportion of those actions to each other was usually distinct. In ten cases it increased, and in five decreased, the proportion of external electric current to "local action" currents. With tin in dilute nitric acid, it greatly increased the proportion of external current to total corrosion, whilst with lead in a solution of potassic hydrate it had the opposite effect. It appeared to increase that proportion the most in cases where much hydrogen was evolved, as with zinc in dilute sulphuric or hydrochloric acid. In seven cases it increased and in nine

decreased the rate of total corrosion of the positive plate, and the amount of the latter was five times that of the former; and in seven instances it increased and in eleven decreased the loss of weight of the comparison plate, and the amount of the latter was fourteen times that of the former.

The rate of total corrosion of the positive plate appeared to be related to the degree of electromotive force. By comparing together Columns 4 and 6 of the table it was found that the instances of least electromotive force and lowest rate of corrosion coincided in the case of magnesium, zinc, tin, aluminium, iron, nickel, and copper; and of greatest coincided in the case of lead and copper. With magnesium the order of electromotive force was almost the same as that of total corrosion, and with copper it was partly so. No. 24 in the table further illustrates the influence of decreased electromotive force.

The simple and chief explanation of the great variation in the proportion of corrosion by "local action" to that attended by external electric current, appears to be difference of electric-conduction-resistance. Where there is no external circuit and the external resistance is infinite, as in cases of deposit of metal or hydrogen by simple immersion, the whole of the electricity flows through the local circuits and deposits its complete equivalent of metal, hydrogen, or both, on the negative parts of the positive plate; but when the external resistance is least, and the resistance to circulation of local currents is greatest, nearly the whole of the electricity passes through the external circuit, and nearly the whole of the deposition of hydrogen or metal occurs on the negative plate. In either case the full equivalent of electricity circulates. With zinc and platinum in dilute sulphuric acid, the "local action" was diminished by closing the circuit. By diminishing also the external resistance in relation to that in the battery by the employment of two voltaic cells in series, instead of one (see No. 24), the proportion of silver deposited was increased from 90·02 to 93·83 per cent. of the theoretical quantity.

Rate of corrosion depends chiefly upon the particular combination of metal and liquid, and is limited by the substance of weakest molecular action. Thus the rates of corrodibility of magnesium in solution of potassic cyanide (or in dilute hydrofluoric acid) and in dilute sulphuric acid, are just the opposite of those of aluminium in the same liquids; while magnesium dissolves very slowly in the former liquid and very rapidly in the latter, aluminium does quite the reverse (see "Relations of Heat to Voltaic and Thermo-Electric Action," "Proc. Roy. Soc.," vol. 36, p. 50); therefore, either a difference of metal or of liquid in this case reverses the effect. Two of the most extreme cases of difference of rate of corrosion observed at 60° F. were aluminium in 3½ per cent. pure hydrofluoric acid, and in very dilute sulphuric acid.

II. "On an Explanation of Hall's Phenomenon." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Professor G. CAREY FOSTER, F.R.S. Received February 14, 1884.

Mr. E. H. Hall's first paper relating to what is now generally known as the "Hall effect," appeared in the "Phil. Mag." for March, 1880. It was entitled "On a New Action of the Magnet on Electric Currents," and in it was described the following experiment:—A strip of gold-leaf was cemented to a plate of glass and placed between the poles of an electromagnet, the plane of the glass being perpendicular to the magnetic lines of force. A current derived from a Bunsen cell was passed longitudinally through the gold, and before the electromagnet was excited two equipotential points were found, by trial, near opposite edges of the gold-leaf and about midway between the ends; when these points were connected with a galvanometer there was, of course, no deflection. A current from a powerful battery being passed through the coils of the magnet, it was found that a galvanometer deflection occurred indicating a difference of potential between the two points, the direction of the current across the gold-leaf being opposite to that in which the gold-leaf itself would have moved across the lines of force had it been free to do so. On reversing the polarity of the magnet, the direction of the transverse electromotive force was reversed; and when the magnet was demagnetised the two points reverted to their original equipotential condition. The phenomenon is attributed by the author to the direct action of the magnet upon the current in the gold leaf.

In a second paper, published in the "Phil. Mag." for November, 1880, experiments are described in which other metals were used in addition to gold. With silver, nickel, tin, and platinum the nature of the effect was the same as with gold though differing in degree; but in the case of iron the direction of the transverse current was found to be reversed, a result which Mr. Hall was naturally inclined to connect with the magnetic properties of iron, and which he says had been predicted by Professor Rowland. But he was greatly puzzled to account for the fact that the two strongly magnetic metals, iron and nickel, gave opposite effects.

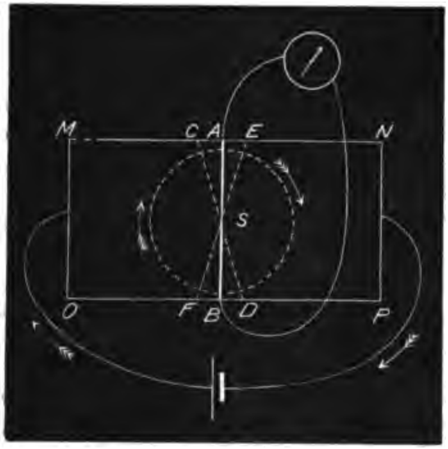
A third paper contained in the "Phil. Mag." for September, 1881, confirmed the previous results with regard to nickel, and added that the direction of the transverse electromotive force in the strongly magnetic metal cobalt was the same as in iron.

Mr. Hall's latest paper on the subject appeared in the "Phil. Mag." for May, 1883, and is entitled, in compliance with a suggestion of Dr. Hopkinson's, "Rotational Coefficients of various Metals." It is here stated that aluminium, copper, and brass behave in the same manner

as gold; zinc gives the same result as iron, and with lead no appreciable effect is produced.

The experimental results may be stated in a different form from that originally adopted by Mr. Hall. The equipotential points on opposite edges of the plate may clearly be joined by equipotential lines, and Hall's results may be expressed by saying that in the case of gold, the equipotential lines are rotated in a direction opposite to that of the current circulating in the coils of the electromagnet, and in the case of iron the lines are rotated in the direction of the magnetising current. In fig. 1 MNOP represents the metal plate

FIG. 1.



through which a battery current is passing from left to right, and AB is the equipotential line (which, if the arrangement is symmetrical, as shown in the figure, will be a straight line) joining two opposite equipotential points. If now the south pole of a magnet is placed beneath the plate and the north pole above it (the axis of the poles being perpendicular to the plate), the equipotential line will be rotated, in the case of gold, counter-clockwise to the position CD, and in the case of iron clockwise to the position EF. Mr. Hall distinguishes the "rotational coefficient" of those metals which behave like iron by the positive sign (+), and those which behave like gold by the negative sign (−).*

Of the thirteen different metals which Hall has tested, three—iron, cobalt, and zinc—are classed as positive; nine—gold, silver, tin, copper, brass, platinum, nickel, aluminium, and magnesium—are negative; and one—lead—shows absolutely no signs of rotation.

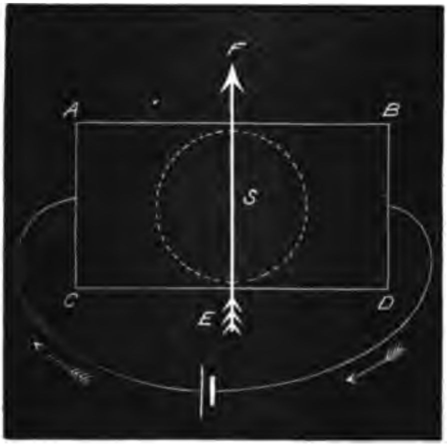
* In the second of Hall's papers the direction of the "transverse electromotive force" is arbitrarily called + for gold, &c., and − for iron. He subsequently adopted opposite signs for their "rotational coefficients."

The several papers abound in speculations which it is not now necessary to consider; but very great importance has been attached to the phenomenon in consequence of the opinion expressed by Professor Rowland and others, that it is connected with the magnetic rotation of the plane of polarisation of light, and thus furnishes additional evidence of an intimate relation between light and electricity.

A number of experiments, which I have from time to time made, convinced me, however, that no direct action of the kind supposed was ever produced; and, after several unsuccessful attempts to frame a hypothesis which would solve the mystery, I finally found that Hall's phenomenon might be completely explained by the joint action of mechanical strain and certain thermoelectric effects.

The mechanical strain is produced by electromagnetic action. In fig. 2, let ABCD represent the metallic strip through which a current

FIG. 2.

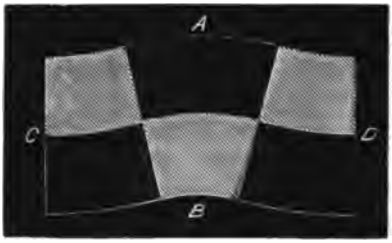


is passing from left to right. Suppose that the south pole of a magnet is beneath it and the north pole above it, then the strip would, if free to do so, move across the lines of force in the direction indicated by the arrow EF.

But, as arranged for Hall's experiment, the metallic strip is fixed, and cannot move bodily from its position; it will, however, be strained in such a manner as to make it tend to assume a form more or less similar to that indicated in fig. 3. As thus distorted, the plate may be mapped out into six districts, in three of which the metal is (on the whole) subjected to longitudinal traction, while in the other three, which are shaded in the figure, it undergoes compression.

Suppose that a current is passing through the plate from C to D, and that A and B are two points on the opposite edges which, when the metal is free from strain, are at the same potential. Then the

Fig. 3.



Compressed portions are shaded. Stretched portions are black.

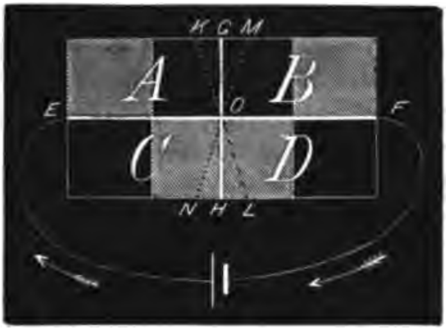
ratio of the resistances between the points C and A and the points A and D is equal to that between the points C, B, and the points B, D. And, so far as mechanical strain alone is concerned, this equality will not be disturbed by placing the plate in a magnetic field, for the strain produced will be symmetrically distributed on both sides of the middle line. At all events, no strain could occur which would in itself affect the resistance of gold and iron in opposite ways, for the resistance of both is increased by tension and (presumably) diminished by longitudinal compression. But it will be noticed that the currents from C to A and from B to D pass from regions which are compressed to regions which are stretched, while the opposite is the case with currents passing from C to B and from A to D. And here the thermoelectric effects above referred to come into play.

Sir William Thomson, in his Bakerian Lecture of 1856 ("Phil. Trans.," 1856, p. 711) announced his discovery of the fact that if a stretched copper wire is connected with an unstretched wire of the same material, and the junction heated, a thermoelectric current will flow from the stretched to the unstretched wire through the hot junction; while if the wires are of iron, the direction of the current will be from unstretched to stretched. From this it might be inferred that a current would flow through the heated junction from an unstretched, or free, copper wire to a longitudinally compressed copper wire, and from a longitudinally compressed iron wire to a free iron wire; and I have ascertained by actual experiment that this is the case, though with copper the observed effect was very small. *A fortiori*, therefore, the direction of the current through the heated junction will be from stretched to compressed in the case of copper wire, and from compressed to stretched in the case of iron wire. If,

therefore, a current is passed from a stretched portion of a wire to a compressed portion, heat will (according to the laws of the Peltier effect) be absorbed at the junction if the metal is copper, and will be developed at the junction if the metal is iron. In passing from compressed to stretched regions the converse effects will occur.

Let us now imagine the metal plate to be divided into four equal regions, A, B, C, D as shown in fig. 4. Let a current pass through the plate from E to F and let a force (produced electromagnetically or otherwise) be applied in the direction HG. First, suppose that the plate is of copper. Then the current travelling from E to the line OG passes from a compressed to a stretched portion of the metal; heat will, therefore, be developed in the region A. Between the line OG and the point F, the current passes from a stretched to a compressed portion; heat will, therefore, be absorbed in the region B. For similar reasons heat will be absorbed in C and developed in D. The temperature of the copper plate will, therefore, not be uniform; the portions A and D being on the whole hotter than the portions B and C. But the resistance of a metal increases with its temperature. The resistance of A and D will, therefore, be greater, and the resistance of B and C smaller than before the plate was subjected to the strain. If, therefore, GH were originally an equipotential line, it is clear that it will be so no longer. An equipotential line through the point O will now be inclined to GH in the direction KL as shown in the figure.*

FIG. 4.



Compressed portions are shaded. Stretched portions are black.

Supposing the plate to be of iron instead of copper, the thermoelectric effects will be reversed, and the regions which in the former case were hot, will now be cold, and *vice versâ*. The distribution of

* The displaced equipotential lines KL, MN, will in point of fact probably be curved, and not straight as drawn.

resistance will be changed in a corresponding manner, and the equipotential line will be rotated in the opposite direction MN.

The peculiar thermoelectric properties of copper and iron discovered by Sir William Thomson are thus seen to be sufficient to account for Hall's phenomena in the case of those metals.

It has often been said that the best test of the truth of a theory is the power it gives of accurate prediction. If the explanation which I have offered of Hall's phenomenon be the correct one, it is clear that Hall's "positive" metals, cobalt and zinc, should when tested according to Sir William Thomson's method exhibit the same thermoelectric effects as iron; that his "negative" metals should behave thermoelectrically in the same manner as copper; and that lead might be expected to be thermoelectrically unaffected by strain. It became

Fig. 5.

exceedingly interesting to ascertain if this was the case, and I therefore proceeded to repeat Thomson's experiment upon all the metals mentioned by Hall. When it was conveniently possible to do so, the metal was used in the form both of wire and of sheet or foil, and except in the case of cobalt, the following simple method of operating was adopted.

A short piece of the wire or strip of the foil to be examined was held at the ends by two fixed brass clamps, about 5 or 6 centims. apart, which were connected with the terminals of a low resistance reflecting galvanometer. A point near the middle of the metal was then gripped by a pair of pincers which had been heated in a Bunsen flame, and one half of the wire was drawn taut, leaving the other half slack; a galvanometer deflection immediately occurred, which ceased when the tension was discontinued. The process was then repeated, the pull being made in the opposite direction, this

resulted in an opposite deflection of the galvanometer. The arrangement is indicated in fig. 5, where A and B are the clamps between which the wire is suspended, C the pincers, and G the galvanometer.

The directions taken by the currents through the hot junctions are given in the following table:—

Table.

S=stretched. U=unstretched.

Metals.	Form used.	Current.	Hall's sign.
Copper	Wire and foil, pure	S to U	−
Iron	Wire and sheet, annealed	U to S	+
Brass	Wire, commercial	S to U	−
Zinc	Wire and foil	U to S	+
Nickel	Wire	S to U	−
Platinum	Wire and foil	S to U	−
Gold	Foil, purity 99·9 per cent.	S to U	−
	Wire, commercially pure	*U to S*	
	Jewellers' 18 ct. wire and sheet.	S to U	
	Jewellers' 15 ct., sheet	S to U	
Silver	Wire and foil	S to U	−
Aluminium	*Wire and foil, pure*	*U to S*	−(?)
Cobalt	Rod, 8 millims. diameter	U to S	+
Magnesium	Ribbon	S to U	−
Tin	Foil	S to U	−
Lead	Foil (assay)	No current.	*Nil.*

It will be seen that in every case, excepting that of aluminium and one out of five specimens of gold, there is a perfect correspondence between the direction of the thermoelectric current and the sign of Hall's coefficient. The anomalous specimen of gold was supplied by Messrs. Johnson and Matthey, by whom its purity was estimated at 99·6 per cent. I do not think that its exceptional behaviour could be accounted for by its physical condition, because, after being successively annealed, raised to a red heat and plunged into cold water, and finally hammered until it was quite hard, it invariably gave the same result. There are, it seems to me, only two possible ways of accounting for it. Either it behaves as if it were absolutely pure gold, in which case the professedly purer piece of foil, which gave the opposite effect, and all the specimens of gold used by Mr. Hall, were affected by impurities; or it is alloyed with some substance which is capable of reversing the ordinary action of pure gold. Since this specimen was unfortunately in the form of wire, it was impossible to test it for the Hall effect.

The aluminium, in the form both of foil and of wire, gave opposite effects to those indicated by the sign of Hall's coefficient. I, therefore, mounted a portion of the foil upon glass, and performed Hall's

experiment with it. As I expected, the sign of the "rotational coefficient" proved to be + like that of iron, zinc, and cobalt. It is probable, therefore, that the specimen of aluminium used by Mr. Hall differed in its chemical or physical constitution from mine. Both my specimens were supplied by Messrs. Johnson and Matthey as pure.

With regard to cobalt, since it could not be obtained in the form either of wire or of sheet, it was necessary for the observation of the thermoelectric effect to treat it in a somewhat different manner from that previously described. A rod of the metal about 8 millims. in diameter and 7 centims. long was firmly held by one end in a vice and both ends were connected by wires with the terminals of the galvanometer. The middle of the rod was gripped with gas-tongs, which had been previously heated in a Bunsen flame, and stress was produced by twisting. It was found that while the stress continued a current passed from the free to the strained portion of the rod through the hot part. The direction of the current was the same whether the torsion was clockwise or counter-clockwise, and it ceased the moment the stress was discontinued. For the sake of comparison a copper rod was treated in a similar manner, and the resulting current was in the reverse direction. There can be little doubt that in experiments of this kind torsion is perfectly equivalent to traction, both tending to draw asunder the molecules of the metal.

I venture to think that the considerations and experiments detailed in this paper render it abundantly evident that the phenomenon described by Mr. Hall involves no new law of nature, but is merely an example of certain thermoelectric effects which had been observed nearly thirty years ago.

February 28, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Structure and Functional Significance of the Human *Corpus callosum.*" By Professor D. J. HAMILTON. Communicated by Professor HUXLEY, P.R.S. Received February 23, 1884.

Summary.

1. The results have been obtained by a special mode of preparation formerly described by me.

2. The fibres in the tip of the frontal lobe run almost directly backwards. A short way behind this they become concentrated in *an oval shaped area*. This *oval shaped area* still further back divides into three distinct bundles, and these bundles I have named the *tractus internus*, *tractus medius*, and *tractus externus*.

3. The *tractus internus* becomes the corpus callosum and contains two sets of fibres, namely, (*a*) those coming from the opposite frontal lobe across the corpus callosum, and (*b*) those going from the frontal lobe of the same side. The fibres which come from the opposite frontal lobe anteriorly do not, after crossing in the corpus callosum, proceed to a corresponding situation on the other side, but, after arching forwards, so as to circumvent the anterior horn of the lateral ventricle and the head of the caudate nucleus, turn back to form the *tractus medius* and *externus*.

4. The *tractus externus* further back divides into the "inner" and "outer capsules," while the *tractus medius* joins with the inner capsule shortly after it is given off from the *tractus externus*. The fibres so derived from the frontal lobe of the opposite side are conducted along with the common fibres of the "inner capsule" to the following destinations. Some of them (a very few twigs) end in the head of the caudate nucleus, while the bulk of them are destined to become attached to the thalamus opticus. One large mass derived

from this source is the anterior peduncle of the optic thalamus. (Vorderer Stiel, Meynert.)

5. The callosal fibres derived from other regions of the cortex pursue a similar course:—They sweep downwards into the corpus callosum, slant somewhat backwards or forwards when they enter it, pass over to the opposite side, and then instead of becoming attached to a corresponding part of the cortex in the opposite hemisphere, turn upwards, outwards, and downwards, in a dense well-marked arch, and enter the inner and outer capsules. This mass of arched fibres I propose to name the *crossed callosal tract.*

6. The fibres of the opposite hemisphere, having thus become united to the inner and outer capsules, terminate in the following manner:—A few of them enter the head of the caudate nucleus, but by far the greater number become attached to the thalamus opticus opposite the bend or knee formed by the junction of the anterior and posterior divisions of the inner capsule. Those entering the outer capsule become attached respectively, from before backwards, with the following parts: the inner capsule, the olfactory tract, the temporo-sphenoidal lobe, and the optic tract. The olfactory and optic nerves are thus, I believe, connected directly with the cerebral cortex.

7. The corpus callosum, therefore, is not an interhemispheric commissure at all, but is the decussation of the cortical fibres in their progress downwards to become connected with the basal ganglia.

In the human brain I have never seen any fibres which pass directly from one hemisphere to another.

8. The "corona radiata," as usually described, does not exist. The only fibres which pass from the cerebral peduncle directly up to the cortex of the same side are contained in the posterior division of the inner capsule. They correspond to those fibres which have decussated already in the anterior pyramids, and are a mere fraction of the whole "inner capsule" fibres. All the other fibres of the "inner capsule" pass into the *crossed callosal tract*, and, instead of running directly up to the cortex of the same side, cross in the corpus callosum to the cortex of the opposite side.

9. The same arrangement prevails in the callosal fibres from the tip of the frontal to the tip of the occipital lobes.

10. The *crossed callosal tract* comes so near to the cortex of the opposite side in arching downwards that injuries or stimulations of one side of the brain would be liable to affect the fibres coming from the opposite, and hence to produce a misleading result.

II. "On the Surface Forces in Fluids." By A. M. WORTHINGTON. Communicated by Professor B. STEWART, F.R.S. Received February 22, 1884.

(Abstract.)

The object of the paper is to deduce from consideration of the known thermal and elastic properties of fluids, the fact that at their bounding surface there is a rapid variation of density, on account of which the surface layers exert either a tension or a pressure on any material wall which cuts them transversely, from which action the phenomena of capillarity are easily deduced; and to exhibit clearly the manner in which the intrinsic energy per unit of volume of the surface layers exceeds or falls short of that in the interior of the fluid, and also to point out a certain important change which it is necessary to introduce into the commonly accepted explanation of the equilibrium of a fluid in contact with a solid; and, finally, to show why the erroneous assumption of a constant density near the surface, made by Laplace, did not prevent his obtaining the true equation to the liquid surface, while it did prevent him from perceiving the reality of the surface-tension.

The first step is to examine the significance of statical theories of molecular equilibrium, and to show that a statical theory, if framed in accordance with the observed phenomena of expansion of the substance with heat, and of its elastic resistance to compression or extension, is really equivalent to a statistical description of any internal dynamical phenomena that are taking place. The value of Poisson's "repulsive force of heat" as a statical representation of the rate of transfer of molecular momentum, or of the internal pressure within the substance, is pointed out, and the nature of the equilibrium between this force and the force of molecular attraction is examined.

The relation of the two forces may be advantageously exhibited by means of a diagram of two curves drawn to rectangular co-ordinates, whose horizontal abscissæ represent the intra-molecular distance, and whose vertical ordinates represent in each case the corresponding value of the force. It is shown that the equilibrium of the substance is stable only when the tangent to the curve of repulsion slopes more steeply than the tangent to the corresponding point of the attraction curve, and the difference between the ordinates to the two curves corresponds to the ordinate of an isothermal curve for the same substance as usually drawn.

The equilibrium of a non-volatile liquid, whose plane surface is exposed to a vacuum, is then studied in the following manner:—

The molecules are supposed at first to be distributed at the equal

distances apart, at which they may be supposed to be situated in the interior of the liquid, at some given temperature in nature. They are then supposed to be endowed with the attractive forces which (on a statical theory) they may be supposed to possess in nature, and the value of the repulsive heat-force or internal pressure is sought which is requisite to maintain the existing uniform density. It is found that this value increases with the depth up to the limit of the radius of molecular activity, after which it remains constant, and that consequently if such a uniform temperature be assigned to the substance as shall maintain the existing density of the interior molecules, a readjustment of the surface molecules will take place, involving an expansion of the surface liquid and an absorption of heat by it, from which it is seen that the surface liquid is in a condition to which the interior liquid would be reduced by stretching it, and may be regarded as a portion of the liquid on which heat energy has been spent in doing work against the molecular attractions.

The same treatment is then extended to the more general case, first of a liquid in contact with an incompressible and inextensible substance; then to two liquids in contact; and then to the contact of a liquid with its own vapour; and finally, to the contact of a fluid and a solid; with the result that at the surface of contact of two fluids the surface layers of each differ from the interior fluid by being in a state either of tension or pressure, and that the forces which are the subject of measurement in experiments on capillarity are the algebraic sum of the two sets of forces belonging to the two fluids respectively. If this resultant sum is a pressure, then the surface is unstable and its extension involves a dissipation of energy.

In the case of a liquid in contact with a gas or its own vapour, it is shown that the liquid is rarefied at the surface while the gas or vapour is condensed, but that there is a limit, for temperatures below the critical point, to the rarefaction possible to the one, and to the condensation possible to the other, and that the two limits are not coincident, and that consequently up to this point the phenomena of a surface-tension are presented, while above the critical temperature the two limits disappear simultaneously. The relation of this interpretation of the critical temperature to the suggestion of Professor James Thomson, made in his paper, "Proc. Roy. Soc.," vol. 20, p. 1, in reference to the continuity of the fluid state of matter, is pointed out.

In the case of the contact of a solid with a fluid it is only the surface forces of the latter that can produce any motion, and when one of the fluids is a gas above its critical temperature, it is shown that the surface force must be a pressure, whence its follows that a liquid which spreads over a solid in air must really be condensed on its surface, and the surface force must be a pressure, and not a tension, as usually supposed—a result which is arrived at in a different

manner by Maxwell ("Encyc. Brit.," art. "Capillarity"), and which is probably of importance when considered in reference to the solution of a solid in a liquid.

The influence of the curvature of the surface is then considered, and the connexion between the hydrostatic pressure and the repulsive and attractive forces that have been spoken of is pointed out, after which the relation of the investigation to that of Laplace is examined, and it is shown that in assuming the density to be invariable, Laplace obliges himself to neglect the very quantities from whose mutual relation, established by experiment, the reality of the surface-tensions and surface-pressures can alone be deduced.

Presents, February 7, 1884.

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"On a Method of Tracing Periodicities in a Series of Observations when the Periods are unknown." By VINAYEK NARAYEU NENE, First Assistant, Government Observatory, Colaba, Bombay. Communicated by C. CHAMBERS, F.R.S., Superintendent of the Observatory. Received May 30. Read June 15, 1882.

[PLATE 4.]

1. The object of this paper is to investigate a new method which will enable us to find out the period of an unknown inequality, and thence the inequality itself, from a series of observations—such as magnetical, meteorological, or astronomical—when they are of a periodic nature.

2. We assume that there is some large complex period, such that the observations—which need not necessarily correspond to equidistant times, but must have the time similarly divided in each general period—will be exactly repeated after such full period;* but since we have not yet found such a period, although we have observations of thirty years' duration or more, say, of barometer, we may say that such a period is not yet completed, and we cannot, therefore, apply Bessel's method, which is applicable to only known periods, to find subordinate periodic inequalities. Diurnal and annual periods which have already been discovered may be considered as subordinate periods of the large period which we are considering. Our aim is first to ascertain the duration of subordinate periods one by one, and then to find out the amplitude for each period.

3. Any observations of a periodic nature may be represented by the equation—

$$Y=u+u_1\sin\left(U_1+\frac{2\pi x}{K_1}\right)+u_2\sin\left(U_2+\frac{2\pi x}{K_2}\right)+, \text{ \&c.} \quad . \quad . \quad . \quad (1),$$

in which K_1, K_2, &c., which are all unknown periods which are in an ascending order of magnitude, but the relations of which with each other are also unknown; Y is the phenomenon, and x the variable time on which the phenomenon depends; u, u_1, u_2, and U_1, U_2, &c., are constants; and 2π is of course equivalent to 360°.

4. The form given in Bessel's paper, embodied in the "Quarterly Weather Report of the Meteorological Department," Part IV, October to December, 1870, is—

$$Y=u+u_1\sin\left(U_1+\frac{2\pi x}{K}\right)+u_2\sin\left(U_2+\frac{4\pi x}{K}\right)+, \text{ \&c.},$$

which form can also be written—

* Practically we shall deal only with observations that subdivide the large period spoken of into equal intervals.

$$Y = u + u_1 \sin\left(U_1 + \frac{2\pi x}{K}\right) + u_2 \sin\left(U_2 + \frac{2\pi x}{\frac{K}{2}}\right) +, \text{\&c.} \quad . \quad . \quad . \quad (2),$$

in which K represents a period, after the completion of which the phenomenon Y constantly recurs, and x is the variable quantity on which the phenomenon depends. The given condition is that Y remains constant for values of x, which are increased or diminished by K, 2K, 3K, &c., u, u_1, u_2, and U_1, U_2, &c., being constants, and 2π equivalent to 360°.

5. Comparing the form of equation (1) and (2) it will be seen that they are both alike, except in subordinate periods which are in equation (1) in an ascending order of magnitude, and apparently unconnected with each other; and in equation (2) they are in a descending order of magnitude, being connected with a proportion 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, &c., in order.

If we suppose that the least common multiple of the subordinate periods K_1, K_2, K_3, &c., in equation (1) is the same as K in equation (2), then the two equations are identical except as to the inversion of the order of the terms, and in that some of the terms of (2) whose coefficients are zero will not appear in equation (1).

6. It might be asked what reasons make us give preference to the form of equation (1) instead of to the form of equation (2); the reply is that the form of equation (2) includes all the subordinate periods that are possible whether they may really exist in a particular series of observations or not, and it is therefore confounding and complicated on account of introducing unnecessary terms in the equation, whereas the form of equation (1) contains only all the unknown subordinate periods, that is, those which may really exist in a particular series of observations. To illustrate practically the advantage of the one over the other in point of simplicity, we shall take a case in which a particular set of observations is composed of subordinate periods of 5, 7, and 15 days only. This can be represented by the form of equation (1) by three terms only, while by the form of equation (2) 105 are required, although it will prove, after all the periods are found, that all of them have *zero* for their value except the terms whose periods are $\frac{1}{7}$, $\frac{1}{15}$, $\frac{1}{21}$ of the full period. On the other hand, if a particular set of observations is really composed of subordinate periods 105 in number, viz., those whose periods are 105, $\frac{105}{2}$, $\frac{105}{3}$, &c., the form of equation (1) will still hold good. It really does not affect the forms but the ideas only.

Having found the equation for any kind of observations of a periodic nature, our next step is to see what results take place if certain operations be performed on the observed values of equation (1).

7. Before doing this let us prove some preliminary propositions.

It has already been established in chapters on the subject of Sum-

mation in Trigonometry that the sum of n terms of the sines of angles which are in arithmetical progression from the first term of the series (3) written below is $u \sin \dfrac{\left(U+\frac{n-1}{2}b\right)\sin\frac{nb}{2}}{\sin\frac{b}{2}}$, and that beginning at the second term it is $u \sin\left\{(U+b)+\frac{n-1}{2}b\right\}\dfrac{\sin\frac{nb}{2}}{\sin\frac{b}{2}}$, and so on. The object of the following investigation is to find what relations exist between the series (3) and the series derived from it, in the manner that we are going to describe below.

Let the proposed series be

$$u\{\sin U+\sin(U+b)+\sin(U+2b)+\sin(U+3b)\ldots\&c.\} \quad . \quad (3),$$

from which we see that the mean of n terms from the first term of the series is $u \sin \dfrac{\left(U+\frac{n-1}{2}b\right)\sin\frac{nb}{2}}{n\sin\frac{b}{2}}$, and that the mean of n terms from the second term of the series is—

$$\frac{u\sin\left\{(U+b)+\frac{n-1}{2}b\right\}\sin\frac{nb}{2}}{n\sin\frac{b}{2}},$$

or, by reducing, we obtain

$$\frac{u\sin\left\{U+\frac{n+1}{2}b\right\}\sin\frac{nb}{2}}{n\sin\frac{b}{2}},$$

and similarly the mean of n terms from the third term of the series is $u\sin\left(U+\frac{n+3}{2}b\right)\dfrac{\sin nb}{n\sin\frac{b}{2}}$, and so on. Thus the mean series will be—

$$u\left\{\sin\left(U+\frac{n-1}{2}b\right)\frac{\sin\frac{nb}{2}}{n\sin\frac{b}{2}}+\sin\left(U+\frac{n+1}{2}b\right)\frac{\sin\frac{nb}{2}}{n\sin\frac{b}{2}}\right.$$

$$\left.+\sin\left(U+\frac{n+3}{2}b\right)\frac{\sin\frac{nb}{2}}{n\sin\frac{b}{2}}+,\ \&c.\right\} \quad . \quad (4.)$$

Again, from this new series we see that the mean of the two terms from the first term of the mean series is $u\sin\left(U+\frac{nb}{2}\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}$, and that the mean of the two terms from the second term of the mean series is $u\sin\left(U+\frac{n+2}{2}b\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}$, and similarly the mean of the two terms from the third term of the mean series is

$$u\sin\left(U+\frac{n+4}{2}b\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}},$$

and so on.

Thus the second mean series will be—

$$u\left\{\sin\left(U+\frac{nb}{2}\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}+\sin\left(U+\frac{n+2}{2}b\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}\right.$$
$$\left.+\sin\left(U+\frac{n+4}{2}b\right)\frac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}+,\ \&c.\right\} \quad . \quad (5).$$

Now in these results we see that the following laws hold—

I. When n is an odd number the first factors of the terms of the first mean series are the same terms in order as the $\left(\frac{n+1}{2}\right)$th term of the proposed series.

II. When n is an even number the first factors of the terms of the second mean series are the same terms in order as the $\left(\frac{n+2}{2}\right)$th term of the proposed series.

III. The second factor in each term of the first or second mean series is a common factor to its own series.

IV. Each first factor of the terms of the first or second mean series is the middle term of the proposed series from which the corresponding mean term is derived.

Thus the investigation established, that the first mean series is

proportional with the proposed series from $\left(\frac{n+1}{2}\right)$th term when n is an odd number, and that the second mean series is proportional with the proposed series from $\left(\frac{n+2}{2}\right)$th term when n is an even number.

8. Suppose in the proposed series that $b=\frac{2\pi}{n}$; then since

$$\sin\frac{nb}{2}=\sin\pi=0,$$

the mean of the sines of the proposed series will be *zero*, and consequently the first mean series or the second mean series will also be *zero*.

9. The factor $\dfrac{\sin\frac{nb}{2}}{n\sin\frac{b}{2}}$ or $\dfrac{\sin\frac{nb}{2}\cos\frac{b}{2}}{n\sin\frac{b}{2}}$ is a proper fraction.

Put $\frac{b}{2}=\text{A}$, thus the first factor becomes $\dfrac{\sin n\text{A}}{n\sin\text{A}}$.

Now $\sin n\text{A}=\sin\{\text{A}+(n-1)\text{A}\}=\sin(n-1)\text{A}\cos\text{A}+\cos(n-1)\text{A}\sin\text{A}$, and $\sin(n-1)\text{A}\cos\text{A}=\sin(n-2)\text{A}\cos^2\text{A}+\cos(n-2)\text{A}\cos\text{A}\sin\text{A}$; and, similarly,

$$\sin(n-2)\text{A}\cos^2\text{A}=\sin(n-3)\text{A}\cos^3\text{A}+\cos(n-3)\text{A}\cos^2\text{A}\sin\text{A}.$$

. . . .

$$\sin 3\text{A}\cos^{(n-3)}\text{A}=\sin 2\text{A}\cos^{(n-2)}\text{A}+\cos 2\text{A}\cos^{(n-3)}\text{A}\sin\text{A}.$$

$$\sin 2\text{A}\cos^{(n-2)}\text{A}=\sin\text{A}\cos^{(n-1)}\text{A}+\cos\text{A}\cos^{n-2}\text{A}\sin\text{A}.$$

Thus

$$\sin n\text{A}=\sin\text{A}\{\cos(n-1)\text{A}+\cos(n-2)\text{A}\cos\text{A}+\cos(n-3)\text{A}\cos^2\text{A}\ldots$$
$$\cos 2\text{A}\cos^{n-3}\text{A}+\cos\text{A}\cos^{n-2}\text{A}+\cos^{n-1}\text{A}\}.$$

The number of terms on the right hand side of the equation is n, and the numerical sum (regardless of sign) of the terms in the bracket is obviously less than n, thus the numerical value of $\sin n\text{A}$ is less than the numerical value of $n\sin\text{A}$; and since $\cos\text{A}$ is numerically less than *unity*, the other factor is also a proper fraction.

10. The following conditions will show without proof when the first and the second factors respectively will have the positive or negative sign :—

The first factor is positive when nb lies between $2\pi(2\text{N})$ and $2\pi(2\text{N}+1)$, and b lies between $2\pi(2\text{N}')$ and $2\pi(2\text{N}'+1)$, or when nb lies between $2\pi(2\text{N}+1)$ and $2\pi(2\text{N}+2)$, and b lies between $2\pi(2\text{N}'+1)$ and $2\pi(2\text{N}'+2)$.

The second factor is positive when nb lies between $2\pi(2N)$ and $2\pi(2N+1)$, and b lies between $\pi(2N')$ and $\pi(2N'+1)$, or when nb lies between $2\pi(2N+1)$ and $2\pi(2N+2)$, and b lies between $\pi(2N'+1)$ and $\pi(2N'+2)$.

The first factor is negative when nb lies between $2\pi(2N+1)$ and $2\pi(2N+2)$, and b lies between $2\pi(2N')$ and $2\pi(2N'+1)$, or when nb lies between $2\pi(2N)$ and $2\pi(2N+1)$, and b lies between $2\pi(2N'+1)$ and $2\pi(2N'+2)$,

The second factor is negative when nb lies between $2\pi(2N+1)$ and $2\pi(2N+2)$, and b lies between $\pi(2N')$ and $\pi(2N'+1)$, or when nb lies between $2\pi(2N)$ and $2\pi(2N+1)$, and b lies between $\pi(2N'+1)$ and $\pi(2N'+2)$, where N and N′ stand for *zero* or any integral number.

11. To trace the changes in the factor $\dfrac{\sin\frac{nb}{2}}{n\sin\frac{b}{2}}$ or $\dfrac{\sin\frac{nb}{2}\cos\frac{b}{2}}{\sin\frac{b}{2}}$ as the angle nb varies between 0 and 2π, between 2π and 4π, between 4π and 6π, &c. We shall discuss this subject in a particular case only, viz., when $b<2\pi$, as there is no necessity of discussing all the cases for our future purpose.

We have already shown that when $b=\dfrac{2\pi}{n}$, or when $nb=2\pi$ the factor has the value *zero*, and it is easy to see that when $nb=4\pi$ or $=6\pi$ or $=2N\pi$, where N is some whole number, the factor has also the value *zero*.

We have now to find what value of nb makes the factor a maximum when $nb<2\pi$. Put $\dfrac{nb}{2}=x$ and therefore $n=\dfrac{2x}{b}$, thus the first factor becomes—

$$\frac{\sin x}{\frac{2x}{b}\sin\frac{b}{2}}=\frac{b\sin x}{2x\sin\frac{b}{2}}:$$

let
$$u'=\frac{b\sin x}{2x\sin\frac{b}{2}},\text{ remove constant factors,}$$

thus
$$u=\frac{\sin x}{x};$$

then
$$\frac{du}{dx}=\frac{x\cos x-\sin x}{x^2},$$

$$\frac{d^2u}{dx^2}=-\frac{\sin x}{x}-\frac{2\cos x}{x^2}+\frac{2\sin x}{x^3}.$$

If we put $\dfrac{du}{dx}=0$ we obtain $\tan x=x$, therefore $x=0$, and when $x=0$

we have

$$\frac{d^2u}{dx^2}=-\frac{\sin x}{x}-\frac{2\cos x}{x^2}+\frac{2\cos x}{3x^2},$$

$$=-\frac{\sin x}{x}-\frac{4\cos x}{3x^2}=-1-\infty.$$

Thus when $x=0$, $\frac{d^2u}{dx^2}$ is negative.

Therefore when $x=0$ u has a maximum value. Thus we see that as the angle nb changes from 0 to 2π, the factor diminishes from the limiting value *unity* to *zero*.

We shall next see when we shall have a minimum value of the factor when nb changes from 2π to 4π.

Put $\frac{nb}{2}=\pi+x$, and consequently $n=\frac{\pi+x}{\frac{b}{2}}$; thus the first factor becomes—

$$\frac{\sin(\pi+x)}{\frac{+x}{\frac{b}{2}}\sin\frac{b}{2}}=\frac{\frac{b}{2}\sin(\pi+x)}{(\pi+x)\sin\frac{b}{2}}=\frac{-\frac{b}{2}\sin x}{(\pi+x)\sin\frac{b}{2}}.$$

Let $$u'=\frac{-\frac{b}{2}\sin x}{(\pi+x)\sin\frac{b}{2}},$$ remove constant factors,

thus $$u=\frac{-\sin x}{\pi+x},$$

then $$\frac{du}{dx}=\frac{-(\pi+x)\cos x+\sin x}{(\pi+x)^2},$$

$$\frac{d^2u}{dx^2}=\frac{\sin x}{\pi+x}+\frac{2\cos x}{(\pi+x)^2}-\frac{2\sin x}{(\pi+x)^3}.$$

If we put $\frac{du}{dx}=0$ we obtain $\tan x=\pi+x$, therefore $x=\frac{77^\circ{\cdot}5}{57^\circ{\cdot}3}$.

Therefore when $x=0$,

$$\frac{d^2u}{dx^2}=\frac{{\cdot}97630}{4{\cdot}493}+\frac{2({\cdot}21644)}{(4{\cdot}493)^2}-\frac{2({\cdot}97630)}{({\cdot}493)^3}=\text{plus something};$$

therefore when $x=\frac{77^\circ{\cdot}5}{57^\circ{\cdot}3}$, u has a minimum value.

Thus when $nb=2\left(\pi+\frac{77^\circ{\cdot}5}{57^\circ{\cdot}3}\right)$ we have a minimum value, and similarly

we can show that when $nb=2\left(2\pi+\frac{82^\circ\cdot 5}{57^\circ\cdot 3}\right)$ we shall have another maximum, and when $nb=2\left(3\pi+\frac{\text{something between } 82\cdot 5 \text{ and } 90}{57\cdot 3}\right)$ we shall have another minimum, and so on alternately; for the general equation from which these values are to be determined is $\tan x = N\pi + x$, where N is *zero* or some integral number.

We have taken the first factor only in dealing with the above subject, but it will be seen by inspection that the same results would have followed if the second factor had been taken.

These changes of factors have been worked out for a particular case, and curved in fig. 1 (Plate 4) to give the reader a general idea of them.

12. Now we shall repeat the equation (1) and proceed as follows:—

$$Y=u+u_1\sin\left(U_1+\frac{2\pi K}{K_1}\right)+u_2\sin\left(U_2+\frac{2\pi K}{K_2}\right)+u_3\sin\left(U_3+\frac{2\pi K}{3}\right)+, \&c.$$

Let c be a small period or interval say of an hour, a day, a week, or a month, &c., as the case may be, at which observations are available, then writing b_1 for $\frac{2\pi c}{K_1}$; b_2 for $\frac{2\pi c}{K_2}$; b_3 for $\frac{2\pi c}{K_3}$, &c., in the above equation (and appropriately modifying the constants U_1, U_2, &c., if x and c are reckoned from different epochs), the successive observations will be equal to the sum of the terms of the following expressions—

1st obs. $=u+u_1\sin U_1+u_2\sin U_2+u_3\sin U_3+$ &c.

2nd obs. $=u+u_1\sin(U_1+b_1)+u_2\sin(U_2+b_2)+u_3\sin(U_3+b_3)+$, &c.

3rd obs. $=u+u_1\sin(U_1+2b_1)+u_2\sin(U_2+2b_2)+u_3\sin(U_3+2b_3)+$, &c.

4th obs. $=u+u_1\sin(U_1+3b_1)+u_2\sin(U_2+3b_2)+u_3\sin(U_3+3b_3)+$, &c.

. . . .

$(n+1)^{\text{th}}=u+u_1\sin(U_1+nb_1)+u_2\sin(U_2+nb_2)+u_3\sin(U_3+nb_3)+$, &c.

In order to save room we shall henceforth use the factor $\frac{1}{q_1}$, $\frac{1}{q_2}$, $\frac{1}{q_3}$, &c., in place of $\frac{\sin\frac{mb_1}{2}}{m\sin\frac{b_1}{2}}$, $\frac{\sin\frac{mb_2}{2}}{m\sin\frac{b_2}{2}}$, $\frac{\sin\frac{mb_3}{2}}{m\sin\frac{b_3}{2}}$, &c., respectively.

13. Suppose we have a table ruled with horizontal and vertical lines; in the first horizontal line let us enter the numerical values of the

observations in order. Now let successive means be taken of m observations (m being an odd integer) advancing to the right by one step at each operation, and entering the respective means under the $\frac{m+1}{2}^{\text{th}}$, $\frac{m+3}{2}^{\text{th}}$, &c., terms of the observations, thus forming a second line of our table; the means in our second line taken in order will be equal to the sum of the terms of the following expressions—

$$\text{1st mean} = u + u_1 \sin\left(\mathrm{U}_1 + \frac{m-1}{2} b_1\right)\frac{1}{q_1} + u_2 \sin\left(\mathrm{U}_2 + \frac{m-1}{2} b_2\right)\frac{1}{q_2}$$
$$+ u_3 \sin\left(\mathrm{U}_3 + \frac{m-1}{2} b_3\right)\frac{1}{q_3} +, \&c.$$

$$\text{2nd mean} = u + u_1 \sin\left(\mathrm{U}_1 + \frac{m+1}{2} b_1\right)\frac{1}{q_1} + u_2 \sin\left(\mathrm{U}_2 + \frac{m+1}{2} b_2\right)\frac{1}{q_2}$$
$$+ u_3 \sin\left(\mathrm{U}_3 + \frac{m+1}{2} b_3\right)\frac{1}{q_3} +, \&c.$$

$$\text{3rd mean} = u + u_1 \sin\left(\mathrm{U}_1 + \frac{m+3}{2} b_1\right)\frac{1}{q_1} + u_2 \sin\left(\mathrm{U}_2 + \frac{m+3}{2} b_2\right)\frac{1}{q_2}$$
$$+ u_3 \sin\left(\mathrm{U}_3 + \frac{m+3}{2} b_3\right)\frac{1}{q_3} +, \&c.$$

. . . .

$$\{n-(m-1)\}^{\text{th}} = u + u_1 \sin\left[\mathrm{U}_1 + \left\{n - \frac{m+1}{2}\right\} b_1\right]\frac{1}{q_1}$$
$$+ u_2 \sin\left[\mathrm{U}_2 + \left\{n - \frac{m+1}{2}\right\} b_2\right]\frac{1}{q_2} + u_3 \sin\left[\mathrm{U}_3 + \left\{n - \frac{m+1}{2}\right\} b_3\right]\frac{1}{q_3} +, \&c.$$

If these means which are entered be subtracted from the observations column by column, then the remainders will severally be equal to the sum of the terms of the following expressions—

$$\text{1st remainder} = u_1 \sin\left(\mathrm{U}_1 + \frac{m-1}{2} b_1\right)\frac{q_1-1}{q_1} + u_2 \sin\left(\mathrm{U}_2 + \frac{m-1}{2} b_2\right)\frac{q_2-1}{q_2}$$
$$+ u_3 \sin\left(\mathrm{U}_3 + \frac{m-1}{2} b_3\right)\frac{q_3-1}{q_3} +, \&c.$$

$$\text{2nd remainder} = u_1 \sin\left(\mathrm{U}_1 + \frac{m+1}{2} b_1\right)\frac{q_1-1}{q_1} + u_2 \sin\left(\mathrm{U}_2 + \frac{m+1}{2} b_2\right)\frac{q_2-1}{q_3}$$
$$+ u_3 \sin\left(\mathrm{U}_3 + \frac{m+1}{2} b_3\right)\frac{q_3-1}{q_3} +, \&c.$$

$$\text{3rd remainder} = u_1 \sin\left(\mathrm{U}_1 + \frac{m+3}{2} b_1\right)\frac{q_1-1}{q_1}$$

$$+u_2 \sin\left(U_2+\frac{m+3}{2}b_2\right)\frac{q_2-1}{q_2}+u_3 \sin\left(U_3+\frac{m+3}{2}b_3\right)\frac{q_3-1}{q_3}+, \text{\&c.}$$

. . . .

$$\{n-(m-1)\}^{\text{th}}=u_1 \sin\left\{U_1+\left(n-\frac{m+1}{2}\right)b_1\right\}\frac{q_1-1}{q_1}$$

$$+u_2 \sin\left\{U_2+\left(n-\frac{m+1}{2}\right)b_2\right\}\frac{q_2-1}{q_2}+u_3 \sin\left\{U_3+\left(n-\frac{m+1}{2}\right)b_3\right\}\frac{q_3-1}{q_3}+, \text{\&c.}$$

Let these remainders be entered in the third line of our table.

Again, if successive means be taken of m of these remainders precisely in the same manner as the means of the observations which were taken before, we shall have a set of second means, and similarly if these means be subtracted from the first set of remainders we shall have a second set of remainders; and, further, if these operations be continued r times we shall have an rth set of means and an rth set of remainders, and the rth set of means taken in order will be equal to the sum of the terms of the following expressions*—

$$\text{1st mean}=u_1 \sin\left(U_1+\frac{m-1}{2}rb_1\right)\frac{(q_1-1)^{r-1}}{q_1^r}$$

$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2\right)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{\&c.}$$

$$\text{2nd mean}=u_1 \sin\left(U_1+\frac{m-1}{2}rb_1+b_1\right)\frac{(q_1-1)^{r-1}}{q_1^r}$$

$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2+b_2\right)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{\&c.}$$

$$\text{3rd mean}=u_1 \sin\left(U_1+\frac{m-1}{2}rb_1+2b_1\right)\frac{(q_1-1)^{r-1}}{q_1^r}$$

$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2+2b_2\right)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{\&c.}$$

. . . .

$$\left\{n-\left(m-1\right)r\right\}^{\text{th}}=u_1 \sin\left\{U_1+\left(n-\frac{m-1}{2}r-1\right)b_1\right\}\frac{(q_1-1)^{r-1}}{q_1^r}$$

$$+u_2 \sin\left\{U_2+\left(n-\frac{m-1}{2}r-1\right)b_2\right\}\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{\&c.}$$

And similarly each of the rth remainders will be represented to be equal to the sum of the terms of the following expressions—

* See two next paragraphs.

$$\text{1st remainder } = u_1 \sin\left(U_1+\frac{m-1}{2}rb_1\right)\frac{(q_1-1)^r}{q_1^r}$$
$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2\right)\frac{(q_2-1)^r}{q_2^r}+,\ \&c.$$

$$\text{2nd remainder} = u_1 \sin\left(U_1+\frac{m-1}{2}rb_1+b_1\right)\frac{(q_1-1)^r}{q_1^r}$$
$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2+b_2\right)\frac{(q_2-1)^r}{q_2^r}+,\ \&c.$$

$$\text{3rd remainder} = u_1 \sin\left(U_1+\frac{m-1}{2}rb_1+2b_1\right)\frac{(q_1-1)^r}{q_1^r}$$
$$+u_2 \sin\left(U_2+\frac{m-1}{2}rb_2+2b_2\right)\frac{(q_2-1)^r}{q_2^r}+,\ \&c.$$

. . . .

$$\{n-(m-1)r\}^{\text{th}} = u_1 \sin\left\{U_1+\left(n-\frac{m-1}{2}r-1\right)b_1\right\}\frac{(q_1-1)^r}{q_1^r}$$
$$+u_2 \sin\left\{U_2+\left(n-\frac{m-1}{2}r-1\right)b_2\right\}\frac{(q_2-1)^r}{q_2^r}+,\ \&c.$$

Pursuing the mode of entry adopted for the first means and first remainders, the rth means will occupy the $2r$th line of our table and the rth remainders the $(2r+1)$th line.

In future we shall for shortness write—

$$U_1+\frac{m-1}{2}rb_1=V_1; \quad U_2+\frac{m-1}{2}rb_2=V_2,\ \&c.$$

14. The following calculations are made for ready reference, and to show how the factors of the form $\frac{(q-1)^{r-1}}{q^r}$ and $\frac{(q-1)^r}{q^r}$ which occurred in our rth means and rth remainders are obtained—

The factor for—

1st means $=\frac{1}{q}$.

1st remainders $=1-\frac{1}{q}=\frac{q-1}{q}$.

2nd means $=\frac{q-1}{q^2}$.

2nd remainders $=\frac{q-1}{q}-\frac{q-1}{q^2}=\frac{(q-1)q-(q-1)}{q^2}=\frac{(q-1)(q-1)}{q^2}$

$$=\frac{(q-1)^2}{q^2}.$$

3rd means $=\dfrac{(q-1)^2}{q^3}$.

3rd remainders $=\dfrac{(q-1)^2}{q^2}-\dfrac{(q-1)^2}{q^3}=\dfrac{(q-1)^2q-(q-1)^2}{q^3}$

$$=\frac{(q-1)^2(q-1)}{q^3}=\frac{(q-1)^3}{q^3}.$$

. . . .

rth means $=\dfrac{(q-1)^{r-1}}{q^r}$.

rth remainders $=\dfrac{(q-1)^{r-1}}{q^{r-1}}-\dfrac{(q-1)^{r-1}}{q^r}=\dfrac{(q-1)^{r-1}q-(q-1)^{r-1}}{q^r}$

$$=\frac{(q-1)^{r-1}(q-1)}{q^r}=\frac{(q-1)^r}{q^r}.$$

15. We must notice that at each operation of taking means we lose $\dfrac{m-1}{2}$ values at each end of the series, thus the rth mean series commences in our form after $\dfrac{m-1}{2}r$ observations. For this reason suppose strong vertical lines are drawn in our form at the $\dfrac{(m-1)}{2}r^{\text{th}}$ observation from each end, and let us confine our attention hereafter to the series embraced between the two strong vertical lines.

16. Then, of the numbers occupying the first vertical column from the first of the strong vertical lines mentioned above, the following, viz., an observation, an individual of the first set of means, an individual of the second set of means, an individual of the third set of means, &c., an individual of the rth set of means, and an individual of the rth set of remainders, will have for their mathematical equivalents the following expressions, taken in order, viz.,

1st observation from the observation series

$$=u+u_1\sin(V_1)+u_2\sin(V_2)+,\ \&c.$$

1st mean from the first set of means

$$=u+u_1\sin(V_1)\frac{1}{q_1}+u_2\sin(V_2)\frac{1}{q_2}+,\ \&c.$$

1st mean from the second set of means

$$=u_1\sin(V_1)\frac{(q_1-1)}{q_1^2}+u_2\sin(V_2)\frac{(q_2-1)}{q_2^2}+,\ \&c.$$

1st mean from the third set of means

$$=u_1 \sin(V_1)\frac{(q_1-1)^2}{q_1^3}+u_2 \sin(V_2)\frac{(q_2-1)^2}{q_2^3}+, \text{ \&c.}$$

. . . .

1st mean from the rth set of means

$$=u_1 \sin(V_1)\frac{(q_1-1)^{r-1}}{q_1^r}+u_2 \sin(V_2)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{ \&c.}$$

1st remainder from rth set of remainder

$$=u_1 \sin(V_1)\frac{(q_1-1)^r}{q_1^r}+u_2 \sin(V_2)\frac{(q_2-1)^r}{q_2^r},$$

and similarly the corresponding numbers from the second vertical column will be represented by the following expressions—

2nd observation from the observation series

$$=u+u_1 \sin(V_1+b_1)+u_2 \sin(V_2+b_2)+, \text{ \&c.}$$

2nd mean from the first set of means

$$=u+u_1 \sin(V_1+b_1)\frac{1}{q_1}+u_2 \sin(V_2+b_2)\frac{1}{q_2}+, \text{ \&c.}$$

2nd mean from the second set of means

$$=u_1 \sin(V_1+b_1)\frac{q_1-1}{q_1^2}+u_2 \sin(V_2+b_2)\frac{(q_2-1)}{q_2^2}+, \text{ \&c.}$$

2nd mean from the third set of means

$$=u_1 \sin(V_1+b_1)\frac{(q_1-1)^2}{q_1^3}+u_2 \sin(V_2+b_2)\frac{(q_2-1)^2}{q_2^3}+, \text{ \&c.}$$

. . . .

2nd mean from the rth set of means

$$=u_1 \sin(V_1+b_1)\frac{(q_1-1)^{r-1}}{q_1^r}+u_2 \sin(V_2+b_2)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{ \&c.}$$

2nd remainder from the rth set of remainders

$$=u_1 \sin(V_1+b_1)\frac{(q_1-1)^r}{q_1^r}+u_2 \sin(V_2+b_2)\frac{(q_2-1)^r}{q_2^r}+, \text{ \&c.}$$

And the corresponding numbers from the $(p+1)$th vertical column will be represented by the following expressions—

$(p+1)$th observation from the observation series

$$=u+u_1 \sin(V_1+pb_1)+u_2 \sin(V_2+pb_2)+, \text{ \&c.}$$

$(p+1)$th mean from the first set of means

$$=u+u_1 \sin(V_1+pb_1)\frac{1}{q_1}+u_2 \sin(V_2+pb_2)\frac{1}{q_2}+, \text{ \&c.}$$

$(p+1)$th mean from the second set of means

$$=u_1 \sin(V_1+pb_1)\frac{q_1-1}{q_1^2}+u_2 \sin(V_2+pb_2)\frac{q_2-1}{q_2^2}+, \text{ \&c.}$$

$(p+1)$th mean from the third set of means

$$=u_1 \sin(V_1+pb_1)\frac{(q_1-1)^2}{q_1^3}+u_2 \sin(V_2+pb_2)\frac{(q_2-1)^2}{q_2^3}+, \text{ \&c.}$$

. . . .

$(p+1)$th mean from the rth set of means

$$=u_1 \sin(V_1+pb_1)\frac{(q_1-1)^{r-1}}{q_1^r}+u_2 \sin(V_2+pb_2)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{ \&c.}$$

$(p+1)$th remainder from the rth set of remainders

$$=u_1 \sin(V_1+pb_1)\frac{(q_1-1)^r}{q_1^r}+u_2 \sin(V_2+pb_2)\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{ \&c.}$$

And therefore if we use a variable integral number P (which may be zero) as a multiple to the angle of the type b, we can make one general expression serve to represent the numbers of any one horizontal line; this will be for the 1st, 3rd, 5th, &c., lines as follows :—

1st line, observation series

$$=u+\{u_1 \sin(V_1+Pb_1)\}+\{u_2 \sin(V_2+Pb_2)\}+, \text{ \&c.}$$

3rd line, 1st mean series

$$=u+\{u_1 \sin(V_1+Pb_1)\}\frac{1}{q_1}+\{u_2 \sin(V_2+Pb_2)\}\frac{1}{q_2}+, \text{ \&c.}$$

5th line, 2nd mean series

$$=\{u_1 \sin(V_1+Pb_1)\}\frac{q_1-1}{q_1^2}+\{u_2 \sin(V_2+Pb_2)\}\frac{q_2-1}{q_2^2}+, \text{ \&c.}$$

7th line, 3rd mean series

$$=\{u_1 \sin(V_1+Pb_1)\}\frac{(q_1-1)^2}{q_1^3}+\{u_2 \sin(V_2+Pb_2)\}\frac{(q_2-1)^2}{q_2^3}+, \text{ \&c.}$$

. . . .

$(2r+1)$th line, rth mean series

$$=\{u_1 \sin(V_1+Pb_1)\}\frac{(q_1-1)^{r-1}}{q_1^r}+\{u_2 \sin(V_2+Pb_2)\}\frac{(q_2-1)^{r-1}}{q_2^r}+, \text{ \&c.}$$

$(2r+2)$th line, rth series of remainders

$$\{u_1 \sin(V_1+Pb_1)\}\frac{(q_1-1)^r}{q_1^r}+\{u_2 \sin(V_2+Pb_2)\}\frac{(q_2-1)^r}{q_2^r}+, \text{ \&c.}$$

Where when P is put $=0, 1, 2$, &c., successively the expression in

each case gives the value of the first, second, third, &c., numbers respectively to the right of the first strong vertical line.

17. We shall now take the sum of all the mean series and put the result by one series only, thus the sum of all the mean series is equal to—

$$u+\{u_1 \sin(V_1+Pb_1)\}\left\{\frac{1}{q_1}+\frac{q_1-1}{q_1^2}+\frac{(q_1-1)^2}{q_1^3}\cdots\frac{(q_1-1)^{r-1}}{q_1^r}\right\}$$

$$+\{u_2 \sin(V_2+Pb_2)\}\left\{\frac{1}{q_2}+\frac{q_2-1}{q_2^2}+\frac{(q_2-1)^2}{q_2^3}\cdots\frac{(q_2-1)^{r-1}}{q_2^r}\right\}$$

$$+\{u_3 \sin(V_3+Pb_3)\}\left\{\frac{1}{q_3}+\frac{q_3-1}{q_3^2}+\frac{(q_3-1)^2}{q_3^3}\cdots\frac{(q_3-1)^{r-1}}{q_3^r}\right\}+, \&c.$$

Suppose $S=\frac{1}{q}+\frac{q-1}{q^2}+\frac{(q-1)^2}{q^3}+\frac{(q-1)^3}{q^4}\cdots\frac{(q_1-1)^{r-1}}{q^r},$

$$=\frac{1}{q}\left\{1+\left(\frac{q-1}{q}\right)+\left(\frac{q-1}{q}\right)^2+\left(\frac{q-1}{q}\right)^3\cdots\left(\frac{q-1}{q}\right)^{r-1}\right\},$$

therefore

$$S\left(\frac{q-1}{q}\right)=\frac{1}{q}\left\{\left(\frac{q-1}{r}\right)+\left(\frac{q-1}{q}\right)^2+\left(\frac{q-1}{q}\right)^3\cdots\left(\frac{q-1}{q}\right)^r\right\}.$$

Hence by subtraction $S\frac{1}{q}=\frac{1}{q}\left\{1-\left(\frac{q-1}{q}\right)^r\right\},$

by division $S=1-\left(\frac{q-1}{q}\right)^r.$

18. From this result we can write the sum of all the mean series—

$$=u+\{u_1 \sin(V_1+Pb_1)\}\left\{1-\left(\frac{q_1-1}{q_1}\right)^r\right\}$$

$$+\{u_2 \sin(V_2+Pb_2)\}\left\{1-\left(\frac{q_2-1}{q_2}\right)^r+, \&c.\right.$$

Thus from one series of observations we get two series, one is the sum of several mean series, and the other the series of last remainders, and of course the sum of these series is equal to the observation series; thus whenever we want the sum of all the mean series we shall get it by subtracting the last remaining series from the corresponding observation series. We shall rewrite these series below and call them *observation series*, *mean series*, and *remaining series* respectively.

Observation series

$$=u+\{u_1 \sin(V_1+Pb_1)\}+\{u_2 \sin(V_2+Pb_2)\}+, \&c. \quad . \quad . \quad . \quad . \quad (6).$$

Mean series

$$=u+\{u_1 \sin(V_1+Pb_1)\}\left\{1-\left(\frac{q_1-1}{q_1}\right)^r\right\}$$

$$+\{u_2 \sin(V_2+Pb_2)\}\left\{1-\left(\frac{q_2-1}{q_2}\right)^r\right\}+, \text{\&c.} \quad . \quad (7).$$

Remaining series

$$=\{u_1 \sin(V_1+Pb_1)\}\left(\frac{q_1-1}{q_1}\right)^r+\{u_2 \sin(V_2+Pb_2)\}\left(\frac{q_2-1}{q_2}\right)^r+, \text{\&c.} \ (8).$$

Throughout these three expressions a particular value of P will correspond to a particular vertical column, the next greater value of P to the next vertical column, and so on.

19. We have up to this point derived these last two results as general with respect to the factors of the type $\frac{1}{q}$ which depend on the value of m, and of this we have only said that it is an odd whole number. Our next step is to see what conclusions we shall arrive at when we apply some particular value to m.

(1.) Suppose that mc is less than K_1, therefore it is also less than each of K_2, K_3, &c., and mb_1, mb_2, &c., are each less than 2π; thus the factors $\dfrac{\sin\frac{mb_1}{2}}{m\sin\frac{b_1}{2}}$, $\dfrac{\sin\frac{mb_2}{2}}{m\sin\frac{b_2}{2}}$, &c., or their equivalents $\frac{1}{q_1}$, $\frac{1}{q_2}$, &c., are positive. We have shown before that each of the above factors is a proper fraction, and consequently each of their equivalents is a proper fraction; thus each of q_1, q_2, &c., is greater than *one*, and therefore the value of each of the factors $\left(\frac{q_1-1}{q_1}\right)^r$, $\left(\frac{q_2-1}{q_2}\right)^r$, &c., when r is sufficiently great reduces to zero.

(2.) Suppose that mc is equal to K_1, therefore it is less than each of K_2, K_3, &c., then, as before, we can show that $\frac{1}{q_1}$ is zero, and each of q_2, q_3, &c., is positive, and the value of $\left(\frac{q_1-1}{q_1}\right)^r$ or $\left(1-\frac{1}{q_1}\right)^r$ is *one*, and the value of each of the factors $\left(\frac{q_2-1}{q_2}\right)^r$, $\left(\frac{q_3-1}{q_3}\right)^r$, &c., when r is sufficiently great, reduces to zero. Thus, when mc is equal to the period K_1, and the number of operations of taking successive means and remainders are sufficiently performed, the remaining series will be reduced to $u_1 \sin(V_1+Pb_1)$, or a subordinate series in the observation whose period is K_1.

(3.) Suppose that mc is greater than K_1 and less than $2K_1$, and

less than each of K_2, K_3, &c., then, as before, we can show that q_1 is negative, and each of q_2, q_3, &c., is positive.

Let $q_1 = -Q_1$, then the factor $\left(\frac{q_1-1}{q_1}\right)^r = \left(1-\frac{1}{q_1}\right)^r$ will be equal to $\left(1+\frac{1}{Q_1}\right)^r = \left(\frac{Q_1+1}{Q_1}\right)^r$, and the value of each of the factors $\left(\frac{q_2-1}{q_2}\right)^r$, $\left(\frac{q_3-1}{q_3}\right)^r$, &c., where r is sufficiently great, reduces to zero. Thus, when mc is greater than the period K_1, less than K_2, and less than double the period of K_1, and the number of operations of taking successive means and remainders are sufficiently performed, the remaining series reduces to $\{u_1 \sin(V_1+Pb_1)\}\left(\frac{Q_1+1}{Q_1}\right)^r$, or a subordinate series in the observation series whose period is K_1, and the value of each term of the series is greater than the corresponding term of the original series: since $\left(\frac{Q_1+1}{Q_1}\right)$ is greater than *one*, and when r is large enough, $\left(\frac{Q_1+1}{Q_1}\right)^r$ can be made as large as we please.

20. Up to this point we have treated the above subject by supposing that m is an odd number. By simple inspection it will be seen that when m is an even number the results will be quite similar to the one when m is an odd number, but the labour of operations of taking successive means will be twofold. Because the mean of m values from which the mean is taken will not correspond to any of the values in the lot, but will correspond to the middle of some two consecutive values, and for this reason the mean should be written on the line instead of in the column on our form; and after the operations of these first means are performed, we must take the mean of two consecutive means found out first, at both sides of any particular column, and put it in the column. Then these second means thus found are to be subtracted from the corresponding values from which the two sets of means are derived. Similarly from these remainders new means and new remainders are to be found precisely in the same manner as spoken of above. In order to keep our language the same whether m is an odd or an even number, we shall always, in the case where m is an even number, give the name of first set of means, second set of means, &c., to those means found by two of the operations described above.

21. This suggests that we must always prefer m as an odd number, except when there is real need of preferring m as an even number, since the labour of operations, when m is an even number, is greater than when m is an odd number. It will be seen further on that there is sometimes real need of taking m as an even number.

22. The three results which we have found above are very important, and we shall now show their utility, and discuss some more cases from them which we have not yet discussed, by taking some particular examples by which we can show our method of tracing unknown periods.

Suppose we have at our disposal a long series of daily means (say) of barometer observations, and we suspect in it a period of about 25 days. The first thing that we shall do is, that we shall take a small series of daily means and curve it on a curve form, and see generally the period of duration of turning points between successive maxima or between successive minima. Suppose the duration of the period found was from 5 to 10 days, then we shall take the least period of 5 days, that is, we shall adopt 5 as our working value of m, and work the processes as already discussed with the whole series of daily means, until generally each mean of the last mean series is less than 2 (say) in the last place of figures.* We shall take then for further treatment the sum of all the mean series instead of the original series. The sum of all the mean series will be found directly by subtracting the last remaining series from the first daily mean series. We shall take this series for a second set of operations. The number we shall take for operations is $25\left(\frac{180+77\cdot5}{180}\right)$=nearly 36, but we shall take 35 because the operations are simpler for odd number than for even number. Now we shall have 35 as our working value of m, by which we shall continue the operations, say up to 5th, and then curve the 5th remaining series on a curve form. If there be only one period in the series from about 17 to 35 days, we shall begin to get simple harmonic waves in the fifth or in some further remaining series; but if there be more than one period the result will be compound, about which we shall discuss presently. But first suppose that there is only one period. If we do not find definitely the duration of simple wave curve in the 5th remaining series, then we shall continue the process on to the 6th remaining series and curve on the same curve form and with the same *zero* line, until the duration of simple wave curve is definitely found. It may here be mentioned, that if the period is not of an exact number of days, we should have a long series to begin with, so that we shall take some exact number of waves from the last remaining series and find the number of days in them, and divide the days by the number of waves, and the result will be the number of days in the period. We must also remark that if there should be no

* We always have some limit of accuracy of observing the instruments and reducing the observed values; for example, we observe barometer to a nearest thousandth of an inch, and also reduce daily means to a nearest thousandth of an inch. By less than two in the last place of figures, in this case, we mean less than two thousandths of an inch of barometer, and so on.

simple harmonic period of exact duration—25 days—that we have been suspecting and searching for, the process we have been performing will still tell us if there be any simple harmonic period of any duration between (say) 20 and 30 days. The only difference we shall have is in these cases that we shall have to increase the number of operations to find the remaining series. In the following table we shall give a few numerical results of the factors—

$$\left\{1-\frac{\sin\frac{35\pi}{20}}{35\sin\frac{\pi}{20}}\right\}^r, \quad \left\{1-\frac{\sin\frac{35\pi}{25}}{35\sin\frac{\pi}{25}}\right\}^r, \text{ and } \left\{1-\frac{\sin\frac{35\pi}{30}}{35\sin\frac{\pi}{30}}\right\}^r.$$

	Numerical values of the factors.		
	$\left\{1-\frac{\sin\frac{35\pi}{20}}{35\sin\frac{\pi}{20}}\right\}^r$	$\left\{1-\frac{\sin\frac{35\pi}{25}}{35\sin\frac{\pi}{25}}\right\}^r$	$\left\{1-\frac{\sin\frac{35\pi}{30}}{35\sin\frac{\pi}{30}}\right\}^r$
When $r=1$	1·1291	1·2168	1·1367
" $r=7$	2·339	3·886	2·452
" $r=11$......	3·802	8·441	4·094
" $r=12$......	4·293	10·25	4·654
" $r=16$......	6·977	22·26	7·770
" $r=18$......	8·895	32·81	10·04
" $r=24$......	18·43	105·0	21·66
" $r=36$......	79·12	1076·0	100·7

The object of introducing the above table is only to give a rough idea to the reader of the rapidity with which the numerical values of these and such similar factors increase after a certain number of operations of the remaining series.

23. Suppose we have suspected only the period of 25 days, but in reality there is not any period between 5 and 35 days; the result will be, as investigated before, that the range of remaining series will be lessened and lessened, until each term of the last remaining series will be less than two in the last place of the figures.

24. Suppose in the daily mean series we have a real existence of the two periods (say) 25 and 20 days, we shall still find the existence of the period of 25 days. Because the numerical value (regardless of sign) of the factor $\dfrac{\sin\frac{35\pi}{25}}{35\sin\frac{\pi}{25}}$ is greater than the numerical value

(regardless of sign) of the factor $\sin \dfrac{\frac{35\pi}{20}}{35 \sin \frac{\pi}{20}}$ (refer to the curve of factors) and consequently $\left(\dfrac{Q_2+1}{Q_2}\right)^r$ can be made as large as we please in comparison with $\left(\dfrac{Q_1+1}{Q_1}\right)^r$ when r is large enough, where $\left(-\dfrac{1}{Q_2}\right)$ stands for the former factor and $\left(-\dfrac{1}{Q_1}\right)$ stands for the latter factor. Thus if the compound result in the last remaining series be curved out on a curve form the maxima and minima of 25 days' period will be apparent. Similarly if any number of compound periods be mixed in the last remaining series, still by sufficiently increasing the number of operations the 25 days' period will be apparent. In conclusion we can say that among the periods which may have mixed in the last remaining series the one of which the factor of the type $\left(\dfrac{1}{q}\right)$ has the greatest numerical value (regardless of sign) will be apparent, whether the period is of 25 days or not.

25. The only case in which the period will not be rendered distinctly apparent is, that when the two periods are such that their factors of the first mean series are equal to one another, and then the two variations will continue combined in the same proportions as in the original observations.

26. We have in the above example stated that when we find systematic movements in the daily mean curve, of which the duration is from 5 to 10 days, we must first work the series with five day operations, &c. It is not, however, essential, when the period which we are seeking is of a considerable range compared with that of the shorter period, then the period can be found without it; but when the range is very small the first process of smoothing is quite essential. It is not also essential that we should stop the operations when the limit of the mean series is reduced to less than 2 in the last place of figures in the series, but we may stop at any stage of the process after considering what reduction will take place in the range of the period we are seeking. This is merely a process for making the series of smooth-flowing numbers without sensibly reducing the range of the larger periods.

27. The process of smoothing series generally in practice is to take the mean of two consecutive numbers, and put it down below and on the line between the two numbers, and so on with each pair of numbers in succession. Next from this new series the same thing is repeated up to a certain number of operations. One effect of this process is to reduce the range of each simple harmonic variation of

which the original series is composed. The extent of this reduction of range is shown in the following expression for the series that is found after S repetitions of such smoothing operations :—

Smoothed observation series*

$$u = + \{u_1 \sin (U_1 + Pb_1)\} \cos^s \frac{b_1}{2} + \{u_2 \sin (U_2 + Pb_2)\} \cos^s \frac{b_2}{2} +, \&c.$$

It will be seen at once that as the period is smaller and smaller, we have greater and greater reduction and *vice versâ*.

28. We shall, however, give a clue when a series contains enormous irregularities, to the distribution of each number amongst the adjacent columns, that is effected by our two methods of smoothing. We shall give this by supposing that we have a series whose terms are 13, of which each of the first and last 6 has the value *zero*, and the 7th has the value 64. The result of six smoothing operations of the second method gives 1, 6, 15, 20, 15, 6, 1. From this we see that the reduction for the middle term which occupies the same column as the number 64 in the original set is rapid, and the distribution amongst the adjacent terms is smooth-flowing and of a convergent character.

29. We shall now apply our first method to a series whose terms are 37, of which each of the first and last 18 has the value *zero*, and the 19th has the value 343. Suppose we have a number 7 for a working value of m to start the operations, we shall after three operations get a series of 19 terms whose values are +1, +3, +6, −11, −27, −42, +91, +75, +57, +37, +57, +75, +91, −42, −27, −11, +6, +3, +1. From this we see also that the reduction is rapid for the middle term, and the distribution amongst the adjacent terms is in smooth-flowing waves which grow smaller and smaller and ultimately vanish.

Method of finding the Amplitude of a Variation of known Period.

30. The processes which we are going to apply for this purpose are

* This may be easily understood if we put $n=2$ in the factor $\dfrac{\sin \frac{nb}{2}}{n \sin \frac{b}{2}}$ of the series 4 in paragraph 7. When $n=2$ the factor becomes $\dfrac{\sin b}{2 \sin \frac{b}{2}} = \dfrac{2 \sin \frac{b}{2} \cos \frac{b}{2}}{2 \sin \frac{b}{2}} = \cos \frac{b}{2}$ for the first operation, and in the same way it becomes $\cos \frac{b}{2} \cos \frac{b}{2}$, *i.e.*, $\cos^2 \frac{b}{2}$ for the second operation, thus for S operations it becomes $\cos^s \frac{b}{2}$. This may be taken as a type of the factor for each expression of series of the type $\{u \sin (U + Pb)\}$.

slightly different from the preceding ones: we shall, therefore, first of all apply them to the general form and then to particular examples.

31. Suppose that $mc = K_n$ where K_n is a known period and m is some integral number. Then using m as working value let us find as before the first mean series from the observation series, the first series of remainders, and the second mean series; but instead of subtracting the terms of the second mean series from the corresponding terms of the first series of remainders to get the second series of remainders, let us add them in the same order and call this series the first *difference and sum series.* Again from this series let us find, in the same manner as stated above, the second, the third, &c., and the rth *difference and sum series.* The rth *difference and sum series* will, as will presently appear, be of the following form—

$$u_1 \sin(V_1 + Pb_1)\left(1 - \frac{1}{q_1^2}\right)^r + u_2 \sin(V_2 + Pb_2)\left(1 - \frac{1}{q_2^2}\right)^r + \ldots\ldots$$

$$u_n \sin(V_n + Pb_n)\left(1 - \frac{1}{q_n^2}\right)^r + u_{n+1} \sin(V_{n+1} + Pb_{n+1})\left(1 - \frac{1}{q_{n+1}^2}\right)^r +, \text{ \&c.} \quad \ldots\ldots (9).$$

32. The following calculations are made for ready reference and to show how the factors of the form $\left(1 - \frac{1}{q^2}\right)^r$, which occurred in our rth *difference and sum series*, are obtained.

The factor for 1st means $= \frac{1}{q}$.

The factor for 1st remainders $= \left(1 - \frac{1}{q}\right)$.

The factor for 2nd means $= \left(1 - \frac{1}{q}\right)\frac{1}{q}$.

The factor for 1st difference and mean series

$$= \left(1 - \frac{1}{q}\right) + \left(1 - \frac{1}{q}\right)\frac{1}{q} = \left(1 - \frac{1}{q}\right)\left(1 + \frac{1}{q}\right) = \left(1 - \frac{1}{q^2}\right).$$

The factor for 3rd means $= \left(1 - \frac{1}{q^2}\right)\frac{1}{q}$.

The factor for 3rd remainders

$$= \left(1 - \frac{1}{q^2}\right) - \left(1 - \frac{1}{q^2}\right)\frac{1}{q} = \left(1 - \frac{1}{q^2}\right)\left(1 - \frac{1}{q}\right).$$

The factor for 4th means $= \left(1 - \frac{1}{q^2}\right)\left(1 - \frac{1}{q}\right)\frac{1}{q}$.

The factor for 2nd difference and sum series

$$=\left(1-\frac{1}{q^2}\right)\left(1-\frac{1}{q}\right)+\left(1-\frac{1}{q^2}\right)\left(1-\frac{1}{q}\right)\frac{1}{q}=\left(1-\frac{1}{q^2}\right)\left(1-\frac{1}{q}\right)\left(1+\frac{1}{q}\right)$$

$$=\left(1-\frac{1}{q^2}\right)\left(1-\frac{1}{q^2}\right)=\left(1-\frac{1}{q^2}\right)^2.$$

Thus it can be shown that the factor for rth *difference and sum series* will be $\left(1-\frac{1}{q^2}\right)^r$.

33. Now the last factors of all the terms in the series (9) contain a term of the type $\frac{1}{q^2}$, so that all these factors are positive, and, as before, it is easy to see that each of them has a tendency to vanish as r increases, except in the cases where the terms of the type $\frac{1}{q^2}$ have the value *zero*. The only terms of the type $\frac{1}{q^2}$, whose values will be *zero*, are those which are formed from the sines of the angles mb_n, $2mb_n$, $3mb_n$, &c., because these angles alone are some integral multiple of 3π. Thus when r is sufficiently great the series (9) will be reduced to—

$$u_n \sin(u_n+\mathrm{P}b_n)+u_{n2}\sin(\mathrm{U}_{n2}+2\mathrm{P}b_n)+u_{n3}\sin(\mathrm{U}_{n3}+3\mathrm{P}b_n)+, \text{ \&c.}$$

If we curved now the first m values in order out of the last horizontal line of our form which forms our rth *difference and sum series*, it will be a variation with its full amplitude of the known period m, and if the next m values be curved in the same way we shall have a repetition of the same curve, and so on.

34. Although we have shown that the factors of the form $\left(1-\frac{1}{q^2}\right)^r$ when $\frac{1}{q^2}$ is not *zero*, and r is sufficiently great, reduce to zero, yet the reduction of the factors which are formed for the periods less than mc is so much slower than the factors which are formed for the periods greater than mc, that it is almost impracticable to continue the process to the limit. We shall therefore in what follows, investigate a particular case of a known period, and we shall find it advantageous to use partly our method and partly the usual method of combining the series with the known period.

35. Suppose we have a series of daily means of barometer observations in which we have detected a period (say) of exactly 36 days, by the method already described. We shall, first of all, in the following table, give factors of the kind in series (9) for different values of r and different values of the periods of the type K.

$\frac{nb}{2}$	90°	108°	257°·4	442°·2	624°·6	805°·8	986°·4	1167°·0	1347°·6
K (Period in days).	72	60	25·2	14·65	10·37	8·04	6·57	5·55	4·81
$\frac{1}{q}$	·63622	·50409	−·21610	+·12640	−·08850	+·06721	−·05348	+·04368	−·03626
$\frac{1}{q^2}$	·40478	·25411	·04670	·01597	·00784	·00452	·00286	·00191	·00131
$1-\frac{1}{q^2}$	·59522	·74789	·95330	·98403	·99216	·99548	·99714	·99809	·99869
$\left(1-\frac{1}{q^2}\right)^5$	·07471	·2309	·78732	·92260	·96140	·97758	·98583	·99049	·99346
$\left(1-\frac{1}{q^2}\right)^{10}$	·00558	·05330	·61987	·85134	·92428	·95567	·97186	·98107	·98697
$\left(1-\frac{1}{q^2}\right)^{15}$	·00042	·01230	·48804	·78551	·88859	·93423	·95808	·97168	·98051

In the above table, the periods less than *mc* of the type K which we have selected are those that have maxima or minima values of their first factors of the type $\frac{1}{q}$ so that their factors of the type $\left(\frac{q^2-1}{q^2}\right)^r$ will have the greatest possible reduction, that is, factors for intermediate periods will reduce at slower rates.

36. We also see that when r is equal to 5, we have the factor corresponding to the period 72 days reduced to $\frac{1}{13}$, and from this we can infer that the factors corresponding to greater periods than 72 days will be reduced to less than $\frac{1}{13}$. So that for all practical purposes we shall make the operations 5, and assume practically that in the fifth *difference and sum series* we have reduced all series of the periods 72 and greater than 72 days to *zero*, and retained in their full magnitude all the series of the periods less than 36 days, whilst the series of periods intermediate between 36 and 72 days will be only partially obliterated. Such a series may be represented by

$$\{u_1 \sin(U_1+Pb_1)+u_2 \sin(U_2+Pb_2)+,\ \&c.\}$$
$$+\left\{u_n \sin\left(U+P\frac{2\pi}{36}\right)+u_{n2} \sin\left(U_{n2}+P\frac{4\pi}{36}\right)+,\ \&c.\right\}$$
$$+\left\{u_{n+1} \sin(U_{n+1}+Pb_{n+1})+u_{n+2} \sin(U_{n+2}+Pb_{n+2})\left(\frac{q_{n+2}^2-1}{q_{n+2}^2}\right)^5+,\ \&c.\right.$$

In the first pair of brackets of the above expression, we have arranged those terms of the simple harmonic series whose periods are less than the period of 36 days, but not any of the period of the form $\frac{36}{N}$ where N is any integral number; in the second pair of brackets of the expression, we have arranged those terms of the

simple harmonic series whose periods are of the form $\frac{36}{N}$, that is, it contains all the harmonics of 36 days; and in the third pair of brackets of the expression, we have arranged those terms of the simple harmonic series whose periods are greater than the period 36 days. Suppose in the fifth *difference and sum series*, or in the last horizontal line of our form, we have 36M values where M is some large integral number. Suppose we have another table ruled with more than M horizontal and 36 vertical lines. In the first horizontal line let us enter 36 numerical values from the commencement of the series in order, in the second horizontal line the next 36 numerical values, and so on, in the Mth horizontal line the last 36 numerical values. Let the sum and mean of each vertical column be taken in the $(m + 1)$th and $(m + 2)$th horizontal lines respectively; then these means are the subject of our present investigation.

37. The general term of the simple harmonic series in the first horizontal line is—

$$u \sin (U + Pb);$$

the general term of the same simple harmonic series in the same vertical column of the second horizontal line is—

$$u \sin (U + Pb + 36b);$$

the general term of the same simple harmonic series in the same vertical column of the third horizontal line is—

$$u \sin \{U + Pb + 2\,(36b)\};$$

the general term of the same simple harmonic series in the same vertical column of the Mth horizontal line is—

$$u \sin \{U + Pb + (M-1)(36b)\};$$

thus the mean of this vertical column will be—

$$u \sin \left\{ (U + Pb) + \frac{M-1}{2} 36b \right\} \frac{\sin M \frac{36b}{2}}{M \sin \frac{36b}{2}},$$

then the general series may be written—

$$u \sin \left\{ \left(U + \frac{M-1}{2} 36b \right) + Pb \right\} \sin \frac{M \frac{36b}{2}}{M \sin \frac{36b}{2}}.$$

Thus the last horizontal line of means in our new form, which is composed of 36 values, will be of the form—

[illegible]	90°	108°	[illegible]	442°·2	624°·6	805°·8	986°·4	1167°·0	[illegible]
λ (Period in days.)	72	60	[illegible]	14·65	[illegible]	8·04	6·57	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	−[illegible]	−[illegible]	−[illegible]	−[illegible]	−·05346	+[illegible]	−[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	·00286	·00191	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

In the above table the periods [illegible] we have [illegible] will have the greatest [illegible] [illegible] periods will [illegible]

[illegible]

[illegible]

simple harmonic series whose [illegible] and so the mean [illegible] appropriate to contains all the harmonics [illegible] in the 9th column. brackets of the expression [illegible] the values will be entered by simple harmonic series whose [illegible] in one column as many 36 days. Suppose in the [illegible] the series in the manner horizontal [illegible] each vertical column are large integral [illegible] example. We will not go more than [illegible] can simply say that the line let us enter [illegible] variation of the period of series in order [illegible] and for 36 points, the values, and [illegible] will be $1\frac{1}{90}$ days. values. Let the [illegible] *difference and sum series* the $(m - 1)$ [illegible] separating the observation series these means are [illegible] series without affecting the

37. The general [illegible] series by the process, we mean horizontal [illegible] *[illegible] series* and *mean series*, if [illegible] value of *m*, and if there

the general term [illegible] $\frac{mc}{2}$ and *mc*, we shall get the vertical column [illegible] the series with a greater amplitude [illegible] separations are very useful for the [illegible] approximate periods.

the general [illegible] vertical column [illegible]

[illegible] Detection of a Period of about 9½ Months.

[illegible] we have taken means of barometric pressure [illegible] to 1872 and 1873 to 1878, thus forming our [illegible] part of monthly means is also given in Table I, [illegible] volume entitled "The Meteorology of the Bombay [illegible] Charles Chambers, Esq., F.R.S., Superintendent of [illegible] Observatory, Bombay; and the last part at the foot of [illegible] pages 58 to 63 of the "Bombay Magnetical and [illegible] Observations, 1871 to 1878." The description of the [illegible] mode of observation, &c., are fully given in both the [illegible] the numbers in Table I, we have performed one [illegible] the first *difference and sum series* by taking 12 as our [illegible] of *m*, and the result is entered in Table II. The [illegible] Table II are subtracted from the corresponding numbers [illegible] and entered in Table III. At the foot of Table II we have [illegible] the mean for January in the 24 years, 1848 to 1871, the mean [illegible] February in the 24 years, the mean for March, &c., &c., up to [illegible] December.* These means form the annual variation of barometric

* [illegible] properly speaking, we should have taken the means of thirty years, 1848 to [illegible] instead of 1848 to 1871; but first of all we have taken for our discussion the

$$\left[u_1 \sin\left\{\left(U_1+\frac{M-1}{2}36b_1\right)+Pb_1\right\}\frac{\sin M\frac{36b_1}{2}}{M\sin\frac{36b_1}{2}}\right.$$

$$\left.+u_2\sin\left\{\left(u_2+\frac{M-1}{2}36\frac{b_2}{2}\right)+Pb_2\right\}\frac{\sin M\frac{36b_2}{2}}{M\sin\frac{36b_2}{2}}\right]$$

$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

$$+\left\{u_n\sin\left(U_n+P\frac{2\pi}{36}\right)+u_{n2}\sin\left(U_{n2}+P\frac{4\pi}{36}\right)+,\ \&c.\right\}$$

$$+\left[u_{n+1}\sin\left\{\left(U_{n+1}+\frac{M-1}{2}36b_{n+1}\right)+Pb_{n+1}\right\}\frac{\sin M\frac{36b_{n+1}}{2}}{M\sin\frac{36b_{n+1}}{2}}\times\left(\frac{q_{n+1}^2-1}{q_{n+1}^2}\right)^r\right.$$

$$\left.+,\ \&c.\right].$$

And it can be shown that the last factors of the terms in the first and the third pairs of brackets, where M is sufficiently great, reduce to zero (see curve of factors in fig. 1). Thus the 36 mean values found in the last horizontal line of our new form will represent the variation of the 36-day period.

38. Next suppose that the period which we have detected is not an exact number of days but (say) of 36·4 days, we shall still take the nearest integral number, viz., 36, as our working value of m, and by it find (say) the 5th *difference and sum series* as already described. In this case the amplitude of the variation of the period of 36·4 days will be slightly reduced in the 5th *difference and sum series*. The reduction of the 1st simple harmonic variation that is of the period of 36·4 days will be (1 — ·99954), that of 18·2 days will be (1 — ·99954), that of 12·13 will be (1 — ·99954), and so on. The next thing is to show how the series is to be combined, which we are going to show in the following manner.

39. In the first horizontal line of our new form, we shall enter first 36 values. In the second horizontal line 8 values, from the beginning of the next 37 values, will be entered up to the 8th vertical column, but in the 9th vertical column we shall enter the mean of 9th and 10th values out of the 37 values intended for the second horizontal line; in the rest of the horizontal line the remaining values will be entered. The object of combining the 9th and 10th values and entering the mean result in the 9th vertical column of the second horizontal line is that, since we are dealing with exact days only, we must enter the values in the columns in such a manner, that the error

of distribution must not be greater than half a day, and so the mean of the 9th and 10th values, which was found more appropriate to 9th column than any other column, was entered in the 9th column. Similarly up to the *m*th horizontal line, the values will be entered by properly combining two values for entering in one column as many times as need be in the course of entering the series in the manner stated above. Also the sums and means of each vertical column are taken in the same way as in the preceding example. We will not go into details, as in the preceding example, but can simply say that the last numbers of our new form will be the variation of the period of 36·4 days with its almost full magnitude, and for 36 points, the intervals between which from point to point will be $1\frac{1}{90}$ days.

40. We should remark that the process of *difference and sum series* has also an obvious advantage in separating the observation series into any number of difference and sum series without affecting the series by the process. By affecting the series by the process, we mean that in the former process for the *remaining series* and *mean series*, if we take any arbitrary number as our working value of m, and if there should be periodicities lying between $\frac{mc}{2}$ and mc, we shall get the periodicities mixed up in both the series with a greater amplitude than the original series. These separations are very useful for the first trials when suspecting approximate periods.

Application of the Process and Detection of a Period of about 9½ Months.

41. For this purpose we have taken means of barometric pressure at Bombay, from 1847 to 1872 and 1873 to 1878, thus forming our Table I. The first part of monthly means is also given in Table I, page 11, of the volume entitled "The Meteorology of the Bombay Presidency," by Charles Chambers, Esq., F.R.S., Superintendent of the Colaba Observatory, Bombay; and the last part at the foot of Tables I to VI, pages 58 to 63 of the "Bombay Magnetical and Meteorological Observations, 1871 to 1878." The description of the barometer and mode of observation, &c., are fully given in both the volumes. With the numbers in Table I, we have performed one operation to get the first *difference and sum series* by taking 12 as our working value of m, and the result is entered in Table II. The numbers in Table II are subtracted from the corresponding numbers in Table 1 and entered in Table III. At the foot of Table II we have entered the mean for January in the 24 years, 1848 to 1871, the mean for February in the 24 years, the mean for March, &c., &c., up to December.* These means form the annual variation of barometric

* Properly speaking, we should have taken the means of thirty years, 1848 to 1877, instead of 1848 to 1871; but first of all we have taken for our discussion the

pressure for the 24 years. If we add the constant 29·808, the annual mean of 26 years, 1847 to 1872, to the 12 numbers in our Table II, then these numbers will be found almost identical with the numbers at the foot of Table I, given in the Meteorology of the Bombay Presidency. The 12 numbers at the foot of Table II were then subtracted in order from each of the rows of 12 numbers commencing from January, and ending with December for each of the years 1848 to 1877 of the same table, and the remainders entered in Table IV. Again, with the numbers in Table IV, we have performed three operations, taking 3 as our working value of m to get the third *difference and sum series*, which we have given in Table V, then the numbers in Table V were subtracted from the corresponding numbers in Table IV, and entered in Table VI.

We need not say that the sum of the numbers in Tables III, V, and VI, from July, 1848, to June, 1876, and successive repetitions of the corresponding numbers of annual variation at the foot of Table II, taken in order, will be equal to the corresponding numbers in Table I. The numbers in Tables III and VI are curved in figs. 2 and 3 respectively. We presume that the curve of fig. 2 is composed of periods of more than 12 months, and that of fig. 3 is of less than 12 months and greater than three months. By looking at the oscillations of the curve of fig. 2 we suspect that amongst the periods there are periods of 11 or 12 years, 4½ years, and 2 years. By looking at the oscillations of the curve of fig. 3, we see that amongst the periods there is a great predominance of a 9½-monthly period. This we have ascertained in the following manner:—We have first of all marked by a cross (thus ×) all the minima that have their time-interval from one minimum to next minimum 8 to 11 months, or any integral multiple of 8 to 11 months, and then filled up the rest 8 to 11-monthly minima that were less marked by a mark (thus 0). It is remarkable that the oscillations are more prominent and of greater amplitude from the year 1849 to 1862 than from the year 1863 to 1876. If we count the marks of minima commencing from July, 1848, and ending with November, 1876, they will be found to be 37; thus there are 36 periods in 340 months, *i.e.*, 9½ months, as an average duration for the period.

42. Again, we find that there are intermediate minima which are of less amplitude than already stated, and the duration of which is also from 8 to 11 months. These also we have marked by marks (* and 0), as in the preceding example. In the present case both these

series from 1847 to 1872, and have made all the operations we are going to describe. But afterwards it was thought proper to add to the above result the six years 1873 to 1878, which were available. But as the work was far advanced it was not worth while to go into the trouble of doing the work over again for the sake of including those six years.

marks are made on the upper side of the curve, while in the preceding case on the lower side of it, to distinguish them from each other.

43. We find from May, 1858, to October, 1866, that the occurrences of these minima have disappeared. From December, 1848, to May, 1858, there are 12 periods, that is $\frac{113}{12}=9\frac{1}{2}$ months for a period, and from October, 1866, to April, 1877, there are 13 periods, *i.e.*, $\frac{126}{13}=9\frac{3}{4}$ months for a period. From this we might guess that to form such a curve there must be two prominent periods, which may be as (say) 5 and $9\frac{1}{2}$ months.

44. To clear this doubt, we have further taken the numbers in Table VI, and performed five operations to get the fifth *difference and sum series* by taking 5 as our working value of *m*. These numbers we have given in Table VII and curved in fig. 4. Then the numbers in Table VII were subtracted from the corresponding numbers in Table VI and entered in Table VIII, and curved in dotted lines with the corresponding curve of fig. 3, and with the same scale and on the same zero line. Now, by looking at the oscillations of the dotted curve, it appears that it is of a simple harmonic character, and it confirms the $9\frac{1}{2}$-monthly period as already described. But in both these curves (continuous and dotted) there are a few exceptions to the confirmation of the $9\frac{1}{2}$-monthly period. These exceptional periods are—from March, 1857, to December, 1859; June, 1863, to September, 1866; April, 1868, to January, 1871; and November, 1872, to the end.

45. We shall now turn to fig. 4. By looking at the oscillations of this curve we again see that amongst the periods there is a great predominance of a $4\frac{3}{4}$-monthly period. This we have ascertained as in the case of $9\frac{1}{2}$-monthly period. If we count all the marks made on the curve, we have 65; thus there are 64 periods in 302 months (from June, 1850, to August, 1875), and $\frac{302}{64}=4{\cdot}72$, *i.e.*, nearly $4\frac{3}{4}$ months,* which is an average duration for the period.

We should now here remark that, although in the continuous curve of fig. 3 we found that the occurrences of the subordinate minima have disappeared from May, 1858, to October, 1866, still they appear in the $4\frac{3}{4}$-monthly period curve of fig. 4.

Application of the Method to find the Duration of an exact Period.

46. The writer had taken a small series of daily means of barometer observations, derived from hourly tabulations of barograph, uncorrected to standard barometer No. 58, from 1st December, 1875, to 31st December, 1876, for trial, when the method of detecting any

* The writer found worth noticing that the period of $4\frac{3}{4}$ months is almost an exact multiple of the $36\frac{1}{2}$-day period which he has found further on in paragraph 50.

simple harmonic waves of an approximately known period was in its infancy. The aim was to see whether on barometer observations there was any influence of lunar period, the duration of which is about 30 days. The trial which we are going to describe was made in the earlier period of 1878, when the barograph tabulation was under consideration. Daily means of barograph tabulations, corrected to standard barometer No. 58, for this same period, may be found in the "Bombay Magnetical and Meteorological Observations, 1871 to 1878," pp. 60 and 61. The difference between the daily means, corrected and uncorrected to standard instrument, is very small, and not varying from day to day more than (say) about two or three thousandths of an inch. The treatment which was given to these daily means and the results which followed from it may be considered satisfactory.

47. In order to get the series in smooth-flowing numbers, we have taken 15 as our working value of m, and found first mean series. Then this series has been taken for further operation, and by taking 45 as our working value of m, we have found first, second, &c., up to fifth series of remainders. The first, third, and fifth series of remainders are curved in fig. 5 in plain, dotted, and interrupted lines respectively, with the same scale and on the same zero line. Looking at the plain curve, we see that there are nine turning-points of minima, the duration thereof being moderately uniform. If we take the first minimum in the middle of the small gap that occurs in the curve, we have in 298 days eight periods. Thus an average duration of one period is about 37 days. Again, if we count the days from the second minimum, we have 258 days in seven waves, and thus an average duration is still about 37 days. Thus, instead of the lunar period of 30 days, we find a 37-day period. From this it is clear that if there be lunar period in the series at all, the amplitude of it must be much smaller than the 37-day period.

48. The object of drawing the three curves on the same zero line and with the same scale is to show graphically the process of swelling the waves as the operations increase and of their appearance, as in the case of the third wave, which disappears in the first series of remainders. The object is also to impress the necessity of making smooth-flowing numbers before taking them for operation; as in the case of second, third, fifth, and seventh waves, all the irregular movements in the first series of remainders are found to appear in the third and fifth series of remainders. With these remarks we conclude without giving the numbers from which the present curves are formed.

Second Application.

49. In order to test the reality of the period of about 37 days on a large scale, we have taken a series of weekly means of barometer observations at Bombay, commencing from 1st January, 1848, and

ending with the 2nd January, 1852, as the year 1848 was the first complete year when the system of hourly observations began without interruption at the Colaba Observatory. The weekly means were chosen in place of daily means for two reasons, first that as no observations were made on Sundays and a few holidays, no continuous daily means were available during the period; second, to save comparatively the labour of calculations of the processes. On some occasions when there were atmospheric disturbances, or for similar reasons, observations were made on Sundays and none on Mondays. For this and similar other reasons the weekly means were calculated from all the daily means from Sunday to Saturday.

These means are given in Table IX, and curved in fig. 6 in an interrupted line. With these weekly means we have performed 5 operations to get the fifth series of remainders. The numbers in the first and fifth series of remainders are also given in Tables X and XI respectively, and curved with the interrupted line of fig. 6, and with the same scale in dotted and continuous lines respectively.

50. Looking at the continuous curve, it will be seen that from 24th week of 1848 to 8th week of 1849, we have 7 waves of simple harmonic character, and equidistant in duration. Thus there are 36 weeks, or 36×7 days in 7 periods, so that the average duration of a period is exactly 36 days. Again, from 8th week of 1849 to 24th week of 1850 there are 13 waves, also almost equidistant in duration. Thus there are 68 weeks, or 68×7 days in 13 periods, so that the average duration of a period is nearly 36·6 days. Again, from 24th week of 1850 to 34th week of 1851 there are, we presume, 12 periods. It will be observed that out of these there are 7 periods, viz., 1st and 7th to 12th, whose waves are quite prominent; with regard to the remaining 5 periods their waves seem to be almost obscured by irregularities. Thus there are 63 weeks, or 63×7 days, in 12 periods, so that the average duration of a period is nearly 36·7 days. If we now add all the weeks and periods together, to get the mean duration of the period we have 167 weeks and 32 periods, therefore the average duration of the period is nearly 36·53 days.

51. We should here remark that as we are dealing with weekly values, there is a possibility of an error, either too much or too little, of one week in the whole period of 167 weeks. If we take 166 weeks for 32 periods, we get an average duration of 36·31 days; and if we take 168 weeks, we get 36·75 days. Thus the period we have found out is approximate to the extent above mentioned. We shall, therefore, say that the period is approximately 36½ days instead of 36·53 days.

52. The object of drawing three curves of weekly means on the same zero line, and with the same scale, is the same as in the preceding example of the curves of daily means. The formation into regular

waves of five weekly period of the continuous curve from the numbers corresponding to the interrupted curve for the periods from 31st to 37th week of 1848, and 13th to 23rd week of 1849 is worth noticing. It should also be noticed that the irregular movements that exist in the interrupted curve for the period from 31st week of 1850 to 15th week of 1851, also exist in the continuous line curve.

53. Let us now combine the result by taking $36\frac{1}{2}$ days as an average duration of the period to get an average range or amplitude of the period, as shown in paragraph 39.

Table showing the variations of $36\frac{1}{2}$-day period for several groups of periods and for the whole period.

Number of points in the curve (7·3 days for a point).	0.	1.	2.	3.	4.
First group of 7 periods	− ·113	− ·016	+ ·114	+ ·081	− ·058
Second group of 13 periods ..	− ·041	− ·034	+ ·018	+ ·034	+ ·016
Third group of 12 periods ...	− ·018	+ ·016	+ ·028	+ ·004	− ·019
Combined above three groups of 32 periods...........	− ·048	− ·011	+ ·041	+ ·033	− ·013
Combined whole series.......	− ·048	− ·012	+ ·036	+ ·035	− ·013

These results are curved in figures 7 to 11 respectively.

It must be mentioned here that the ranges which we see in these results are not exact, but enlarged by our process and $\left\{1-\dfrac{\sin 7\dfrac{7\pi}{36\frac{1}{2}}}{\sin 7\dfrac{7\pi}{36\frac{1}{2}}}\right\}^5$ = nearly 2·7, is the extent to which our results are enlarged.

But we must also notice that the facts that the period is not an integral number of weeks, that our method of combination is in consequence rather rough, and that the number of points in the curve is too small, tend to reduce the range of the period. If the process had been applied to the continuous daily means instead of to weekly means the results would have been of a larger and equal amplitude of several groups of the period.

54. The writer feels himself under great obligation to his superior, C. Chambers, Esq., F.R.S., Superintendent of the Bombay Observatory, for the warm interest he took in carefully reading this tract, and in giving here and there some very valuable suggestions.

Table I.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1847...	29·917	29·903	29·872	29·791	29·722	29·621	29·651	29·707	29·748	29·825	29·893	29·897
1848...	·927	·916	·830	·770	·741	·649	·654	·706	·812	·826	·909	·922
1849...	·945	·906	·870	·779	·714	·624	·672	·700	·727	·841	·889	·911
1850...	·878	·927	·885	·816	·763	·639	·643	·736	·786	·785	·891	·949
1851...	·968	·901	·857	·779	·739	·631	·599	·697	·782	·828	·838	·946
1852...	·929	·903	·829	·794	·746	·630	·660	·731	·756	·856	·905	·923
1853...	·940	·873	·859	·797	·775	·644	·673	·742	·793	·854	·863	·955
1854...	·935	·921	·885	·792	·760	·674	·618	·712	·715	·785	·923	·933
1855...	·935	·932	·853	·818	·787	·663	·658	·743	·787	·826	·939	·944
1856...	·985	·922	·835	·787	·718	·651	·610	·707	·799	·834	·897	·932
1857...	·930	·869	·831	·790	·735	·640	·649	·681	·811	·857	·918	·981
1858...	·921	·939	·852	·798	·674	·698	·670	·720	·778	·822	·927	·944
1859...	·933	·918	·878	·787	·789	·662	·628	·731	·779	·854	·851	·931
1860...	·953	·875	·845	·827	·764	·650	·631	·725	·735	·809	·917	·919
1861...	·907	·897	·857	·764	·713	·666	·639	·689	·774	·857	·867	·932
1862...	·920	·871	·862	·801	·786	·634	·625	·689	·693	·780	·853	·887
1863...	·939	·883	·838	·746	·734	·606	·637	·697	·767	·811	·895	·947
1864...	·945	·923	·887	·821	·815	·674	·660	·742	·794	·867	·920	·931
1865...	·972	·910	·874	·800	·741	·669	·656	·666	·788	·848	·890	·918
1866...	·959	·905	·867	·916	·775	·675	·661	·706	·795	·808	·927	·969
1867...	·965	·916	·879	·819	·734	·671	·667	·692	·750	·826	·983	·981
1868...	·937	·921	·881	·831	·796	·673	·699	·742	·817	·857	·920	·963
1869...	·966	·936	·879	·821	·773	·659	·668	·705	·730	·824	·908	·901
1870...	·879	·877	·836	·795	·764	·651	·637	·724	·754	·807	·900	·936
1871...	·894	·891	·863	·793	·768	·641	·672	·733	·779	·821	·870	·932
1872...	·932	·907	·850	·776	·767	·640	·658	·677	·760	·820	·866	·892
1873...	·924	·895	·859	·803	·733	·646	·641	·742	·788	·824	·935	·943
1874...	·975	·925	·857	·826	·720	·628	·655	·720	·740	·801	·920	·953
1875...	·918	·905	·849	·780	·772	·652	·655	·720	·754	·833	·923	·938
1876...	·922	·904	·868	·758	·763	·683	·631	·728	·802	·875	·896	·957
1877...	·976	·936	·887	·827	·790	·723	·745	·762	·808	·865	·921	·917
1878...	·953	·953	·902	·820	·783	·683	·654	·665	·688	·780	·836	·882

Table II.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1848...	+ ·130	+ ·118	+ ·031	− ·031	− ·061	− ·154	− ·150	− ·099	+ ·006	+ ·020	+ ·103	+ ·116
1849...	+ ·141	+ ·102	+ ·068	− ·022	− ·086	− ·175	− ·125	− ·099	− ·072	+ ·042	+ ·088	+ ·110
1850...	+ ·076	+ ·124	+ ·080	+ ·010	− ·044	− ·167	− ·164	− ·071	− ·020	− ·020	+ ·086	+ ·144
1851...	+ ·155	+ ·100	+ 058	− ·020	− ·058	− ·165	− ·196	− ·098	− ·013	+ ·033	+ ·042	+ ·150
1852...	+ ·133	+ ·106	+ ·030	− ·006	− ·055	− ·173	− ·144	− ·074	− ·050	+ ·049	+ ·097	+ ·114
1853...	+ ·131	+ ·063	+ ·048	− ·016	− ·038	− ·170	− ·142	− ·074	− ·023	+ ·039	+ ·049	+ ·141
1854 ..	+ ·122	+ ·110	+ ·075	− ·017	− ·048	− ·133	− ·187	− ·092	− ·090	− ·021	+ ·115	+ ·124
1855...	+ ·125	+ ·120	+ ·038	+ ·002	− ·031	− ·156	− ·162	− ·077	− ·033	+ ·007	+ ·120	+ ·128
1856...	+ ·170	+ ·110	+ ·025	− ·022	− ·089	− ·153	− ·194	− ·095	− ·002	+ ·034	+ ·097	+ ·133
1857...	+ ·131	+ ·069	+ ·030	− ·013	− ·070	− ·166	− ·158	− ·128	+ ·001	+ ·046	+ ·106	+ ·168
1858 .	+ ·107	+ ·125	+ ·038	− ·016	− ·140	− ·116	− ·144	− ·095	− ·036	+ ·009	+ ·112	+ ·129
1859 ..	+ ·118	+ ·104	+ ·064	− ·027	− ·025	− ·151	− ·184	− ·080	− ·031	+ ·046	+ ·043	+ ·125
1860...	+ ·147	+ ·070	+ ·040	+ ·024	− ·039	− ·152	− ·170	− ·075	− ·064	+ ·010	+ ·119	+ ·122
1861...	+ ·111	+ ·101	+ ·061	− ·032	− ·084	− ·130	− ·158	− ·109	− ·025	+ ·059	+ ·069	+ ·135
1862...	+ ·124	+ ·076	+ ·068	+ ·009	− ·004	− ·154	− ·161	− ·096	− ·089	+ ·001	+ ·073	+ ·107
1863...	+ ·159	+ ·102	+ ·057	− ·037	− ·052	− ·183	− ·155	− ·098	− ·033	+ ·006	+ ·086	+ ·135
1864...	+ ·129	+ ·104	+ ·065	− ·003	− ·012	− ·154	− ·169	− ·087	− ·034	+ ·039	+ ·094	+ ·106
1865...	+ ·150	+ ·090	+ ·057	− ·016	− ·073	− ·144	− ·156	− ·146	− ·024	+ ·037	+ ·079	+ ·105
1866...	+ ·145	+ ·091	+ ·051	− ·001	− ·043	− ·145	− ·159	− ·114	− ·025	− ·012	+ ·106	+ ·148
1867...	+ ·144	+ ·096	+ ·058	− ·002	− ·086	− ·150	− ·155	− ·130	− ·074	+ ·000	+ ·156	+ ·152
1868...	+ ·107	+ ·089	+ ·047	− ·004	− ·040	− ·164	− ·138	− ·096	− ·021	+ ·021	+ ·086	+ ·131
1869...	+ ·136	+ ·109	+ ·055	·000	− ·044	− ·155	− ·142	− ·102	− ·074	+ ·022	+ ·108	+ ·107
1870...	+ ·083	+ ·081	+ ·040	·000	− ·031	− ·145	− ·160	− ·074	− ·044	+ ·008	+ ·099	+ ·133
1871...	+ ·092	+ ·087	+ ·059	− ·012	− ·037	− ·164	− ·134	− ·073	− ·027	+ ·016	+ ·065	+ ·127
1872...	+ ·128	+ ·104	+ ·049	− ·024	− ·032	− ·158	− ·139	− ·119	− ·035	+ ·024	+ ·071	+ ·096
1873...	+ ·127	+ ·096	+ ·058	·000	− ·072	− ·161	− ·168	− ·069	− ·026	+ ·009	+ ·120	+ ·129
1874...	+ ·161	+ ·111	+ ·044	+ ·015	− ·089	− ·180	− ·152	− ·086	− ·065	− ·004	+ ·115	+ ·148
1875...	+ ·113	+ ·100	+ ·043	− ·026	− ·034	− ·155	− ·152	− ·088	− ·054	+ ·024	+ ·115	+ ·129
1876...	+ ·112	+ ·093	+ ·056	− ·055	− ·052	− ·135	− ·189	− ·094	− ·023	+ ·047	+ ·065	+ ·123
1877...	+ ·139	+ ·097	+ ·046	− ·016	− ·055	− ·123	− ·100	− ·083	− ·035	+ ·024	+ ·083	+ ·083
Mean...	+ ·128	+ ·098	+ ·052	− ·010	− ·054	− ·155	− ·159	− ·095	− ·037	+ ·020	+ ·092	+ ·129

Table III.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1848...	29·797	29·798	29·799	29·801	29·802	29·803	29·804	29·805	29·806	29 806	29·806	29·806
1849...	·804	·804	·802	·801	·800	·799	·797	·799	·799	·799	·801	·801
1850...	·802	·803	·805	·806	·807	·806	·807	·807	·806	·805	·805	·805
1851...	·803	·801	·799	·799	·797	·796	·795	·795	·795	·795	·796	·796
1852...	·796	·797	·799	·800	·801	·803	·804	·805	·806	·807	·808	·809
1853...	·809	·810	·811	·813	·813	·814	·815	·816	·816	·815	·814	·814
1854...	·813	·811	·810	·809	·808	·807	·805	·804	·805	·806	·808	·809
1855...	·810	·812	·815	·816	·818	·819	·820	·820	·820	·819	·819	·816
1856...	·815	·812	·810	·809	·807	·804	·804	·802	·801	·800	·800	·799
1857...	·799	·800	·801	·803	·805	·806	·807	·809	·810	·811	·812	·813
1858...	·814	·814	·814	·814	·814	·814	·814	·815	·814	·813	·815	·815
1859...	·815	·814	·814	·814	·814	·813	·812	·811	·810	·808	·808	·806
1860...	·806	·805	·805	·803	·803	·802	·801	·800	·799	·799	·798	·797
1861..	·796	·796	·796	·796	·797	·796	·797	·798	·799	·798	·798	·797
1862...	·796	·795	·794	·792	·790	·788	·786	·784	·782	·781	·780	·780
1863...	·780	·781	·781	·783	·786	·789	·792	·795	·800	·805	·809	·812
1864...	·816	·819	·822	·824	·827	·828	·829	·831	·828	·828	·826	·823
1865...	·822	820	·817	·816	·814	·813	·812	·812	·812	·811	·811	·813
1866...	·814	·814	·816	·817	·818	·820	·820	·820	·820	·820	·821	·821
1867...	·821	·820	·821	·821	·820	·821	·822	·822	·824	·825	·827	·829
1868...	·830	·832	·834	·835	·836	·837	·837	·838	838	·836	·834	·832
1869	·830	·827	·824	·821	·817	·814	·810	·807	·804	·802	·800	·797
1870...	·796	·796	·796	·795	·795	·796	·797	·798	·798	·799	·801	·803
1871...	·802	·804	·804	·805	·805	·805	·806	·806	·806	·805	·805	·805
1872...	·804	·803	·801	·800	·799	·798	·797	·796	·795	·796	·795	·796
1873...	·797	·799	·801	·803	·805	·807	·809	·811	·814	·815	·815	·814
1874...	·814	·814	·813	·811	·809	·808	·807	·806	·805	·805	·805	·805
1875...	·805	·805	·806	·806	·806	·807	·807	·808	·808	·809	·808	·809
1876...	·810	·811	·812	·813	·815	·818	·820	·822	·825	·828	·831	·834
1877...	·837	·839	·841	·843	·845	·846	·845	·845	·843	·641	·838	·834

Table IV.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1848...	+ ·002	+ ·020	− ·021	− ·021	− ·007	+ ·001	+ ·009	− ·004	+ ·043	·000	+ ·011	− ·013
1849...	+ ·013	+ ·004	+ ·016	− ·012	− ·032	− ·020	+ ·034	− ·004	− ·035	− ·022	− ·004	− 019
1850...	− ·052	+ ·026	+ ·028	+ ·020	+ ·010	− ·012	− ·005	+ ·024	+ ·017	− ·040	− ·006	+ ·015
1851...	+ ·027	+ ·002	+ ·006	− ·010	− ·004	− ·010	− ·037	− ·003	+ ·024	+ ·013	− ·060	+ ·021
1852...	+ ·005	+ ·008	− ·022	+ ·004	− ·001	− ·018	+ ·015	+ ·021	− ·013	+ ·029	+ ·005	− ·015
1853...	+ ·003	− ·035	− ·004	− ·006	+ ·016	− ·015	+ ·017	+ ·021	+ ·014	+ ·019	− ·043	+ ·012
1854...	− ·006	+ ·012	+ ·023	− ·007	+ ·006	+ ·022	− ·028	+ ·003	− ·057	− ·041	+ ·023	− ·005
1855...	− ·003	+ ·022	− ·014	+ ·012	+ ·023	− ·001	− ·003	+ ·018	+ ·004	− ·013	+ ·028	− ·001
1856...	+ ·042	+ ·012	− ·027	− ·012	− ·035	+ ·002	− ·035	·000	+ ·035	+ ·014	+ ·005	+ ·004
1857...	+ ·003	− ·029	− ·022	− ·003	− ·016	− ·011	+ ·001	− ·033	+ ·038	+ ·026	+ ·014	+ ·039
1858...	− ·021	+ ·027	− ·014	− ·006	− ·086	+ ·039	+ ·015	·000	+ ·001	− ·011	+ ·020	·000
1859...	− ·010	+ ·006	+ ·012	− ·017	+ ·029	+ ·004	− ·025	+ ·015	+ ·006	+ ·026	− ·049	− ·004
1860...	+ ·019	− ·028	− ·012	+ ·034	+ ·015	+ ·003	− ·011	− ·020	− ·027	− ·010	+ ·027	− ·007
1861...	− ·017	+ ·003	+ ·009	− ·022	− ·030	+ ·020	+ ·001	− ·014	+ ·012	+ ·039	− ·023	+ ·006
1862...	− ·004	− ·022	+ ·016	+ ·019	+ ·050	+ ·001	− ·002	·000	− ·052	− ·019	− ·019	− ·022
1863...	+ ·031	+ ·004	+ ·005	− ·027	+ ·002	− ·028	+ ·004	− ·003	+ ·004	− ·014	− ·006	+ ·006
1864...	+ ·001	+ ·006	+ ·013	+ ·007	+ ·042	+ ·001	− ·010	+ ·008	+ ·003	+ ·019	+ ·002	− ·021
1865...	+ ·022	− ·008	+ ·005	− ·006	− ·019	+ ·011	+ ·003	− ·051	+ ·013	+ ·017	− ·013	− ·024
1866...	+ ·017	− ·007	− ·001	+ ·009	+ ·011	+ ·010	·000	− ·019	+ ·012	− ·032	+ ·014	+ ·019
1867...	+ ·016	− ·002	+ ·006	+ ·008	− ·032	+ ·005	+ ·004	− ·035	− ·037	− ·019	+ ·064	+ ·023
1868...	− ·021	− ·009	− ·005	+ ·006	+ ·014	− ·009	+ ·021	− ·001	+ ·016	+ ·001	− ·006	+ ·002
1869...	+ ·008	+ ·011	+ ·003	+ ·010	+ ·010	·000	+ ·017	− ·007	− ·037	+ ·002	+ ·016	− ·022
1870...	− ·045	− ·017	− ·012	+ ·010	+ ·023	+ ·010	− ·001	+ ·021	− ·007	− ·012	+ ·007	+ ·004
1871...	− ·036	− ·011	+ ·007	− ·002	+ ·017	− ·009	+ ·025	+ ·022	+ ·010	− ·004	− ·027	− ·002
1872...	·000	+ ·006	− ·003	− ·014	+ ·022	− ·033	+ ·020	− ·024	− ·002	+ ·004	− ·021	− ·033
1873...	− ·001	− ·002	+ ·006	+ ·010	− ·018	− ·006	− ·009	+ ·026	+ ·011	− ·011	+ ·028	·000
1874...	+ ·033	+ ·013	− ·008	+ ·025	− ·035	− ·025	+ ·007	+ ·009	− ·028	− ·024	+ ·023	+ ·019
1875...	− ·015	+ ·002	− ·009	− ·016	+ ·020	·000	+ ·007	+ ·007	− ·017	+ ·004	+ ·023	·000
1876...	− ·016	− ·005	+ ·004	− ·045	+ ·002	+ ·020	− ·030	+ ·001	+ ·014	+ ·027	− ·027	− ·006
1877...	+ ·011	− ·001	− ·006	− ·006	− ·001	+ ·032	+ ·059	+ ·012	+ ·002	+ ·004	− ·009	− ·046

Table V.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1848...	...	...	...	...	...	...	+ ·001	− ·014	+ ·021	− ·010	+ ·004	− ·008
1849...	+ ·006	− ·005	+ ·010	+ ·003	− ·013	− ·011	+ ·030	− ·005	− ·028	+ ·021	+ ·007	·000
1850...	− ·027	+ ·018	+ ·013	− ·004	+ ·004	− ·008	− ·011	+ ·014	+ ·016	− ·025	+ ·001	+ ·008
1851...	+ ·006	− ·009	+ ·004	− ·006	+ ·005	+ ·009	− ·017	− ·004	+ ·019	+ ·009	− ·036	+ ·022
1852...	+ ·003	+ ·001	− ·015	+ ·010	+ ·006	− ·017	+ ·011	+ ·013	− ·014	− ·006	+ ·007	− ·005
1853...	+ ·016	− ·015	+ ·003	− ·003	+ ·012	− ·014	+ ·001	+ ·003	·000	+ ·014	− ·028	+ ·019
1854...	− ·007	− ·001	+ ·016	− ·015	·000	+ ·017	− ·021	+ ·025	− ·019	− ·015	+ ·029	− ·012
1855...	− ·009	+ ·018	− ·018	+ ·001	+ ·013	− ·008	− ·009	+ ·014	− ·002	− ·012	+ ·014	− ·020
1856...	+ ·015	+ ·003	− ·015	+ ·008	− ·009	+ ·021	− ·017	− ·006	+ ·018	− ·007	− ·006	+ ·002
1857...	+ ·009	− ·011	− ·002	+ ·011	− ·007	+ ·003	+ ·011	− ·028	+ ·022	− ·001	− ·010	+ ·019
1858...	− ·026	+ ·015	+ ·001	+ ·014	− ·044	+ ·036	+ ·002	− ·011	+ ·003	− ·009	+ ·014	− ·001
1859...	− ·011	+ ·006	+ ·008	− ·023	+ ·020	+ ·005	− ·017	+ ·002	+ ·002	+ ·023	− ·035	+ ·005
1860...	+ ·028	− ·022	− ·010	+ ·019	− ·004	− ·005	+ ·002	+ ·004	− ·009	− ·005	+ ·021	− ·006
1861...	− ·014	+ ·006	+ ·015	− ·009	− ·016	+ ·024	− ·001	− ·019	+ ·005	+ ·024	− ·027	+ ·009
1862...	+ ·007	− ·014	+ ·006	− ·004	+ ·018	− ·016	− ·001	+ ·018	− ·019	+ ·009	+ ·005	− ·014
1863...	+ ·019	− ·004	+ ·001	− ·010	+ ·015	− ·014	+ ·006	+ ·001	+ ·005	− ·007	·000	+ ·007
1864...	− ·003	+ ·001	·000	− ·011	+ ·019	− ·006	− ·009	+ ·008	− ·006	+ ·010	·000	− ·016
1865...	+ ·019	− ·009	+ ·004	+ ·001	− ·016	+ ·017	+ ·012	− ·034	+ ·015	+ ·017	− ·010	− ·013
1866...	+ ·019	− ·006	− ·005	+ ·003	− ·001	+ ·004	− ·002	− ·009	+ ·025	− ·017	− ·007	+ ·010
1867...	+ ·005	− ·012	+ ·004	+ ·013	− ·024	+ ·007	+ ·014	− ·004	− ·008	− ·020	+ ·034	+ ·001
1868...	− ·021	+ ·006	+ ·002	+ ·002	+ ·006	− ·012	+ ·011	− ·008	+ ·006	·000	− ·005	·000
1869...	+ ·003	+ ·003	+ ·005	+ ·003	·000	− ·009	+ ·014	+ ·004	− ·022	+ ·006	+ ·019	− ·007
1870...	− ·013	+ ·011	− ·003	− ·002	+ ·009	− ·005	− ·009	+ ·016	− ·006	− ·012	+ ·010	+ ·011
1871...	− ·019	+ ·002	+ ·009	− ·005	+ ·009	− ·015	+ ·006	+ ·003	+ ·002	+ ·003	− ·012	+ ·006
1872...	·000	+ ·005	− ·001	− ·010	+ ·012	− ·010	+ ·014	− ·017	+ ·002	+ ·014	− ·005	− ·012
1873...	+ ·011	− ·003	− ·001	+ ·009	− ·008	+ ·001	− ·008	+ ·015	·000	− ·018	+ ·014	− ·007
1874...	+ ·012	− ·005	− ·016	+ ·028	− ·016	− ·012	+ ·013	+ ·014	− ·014	− ·014	+ ·018	+ ·006
1875...	− ·017	+ ·009	·000	− ·013	+ ·015	− ·008	·000	+ ·009	− ·014	− ·002	+ ·014	·000
1876...	− ·014	+ ·007	+ ·018	− ·030	+ ·009	+ ·023	− ·022	− ·003	+ ·006	+ ·017	− ·021	− ·001
1877...	+ ·013	− ·002	·000	+ ·001	− ·010	·000						

Table VI.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1848...	...	...	...	...	...	...	+ ·008	+ ·010	+ ·002	+ ·010	+ ·007	− ·005
1849...	+ ·007	+ ·009	+ ·006	− ·015	− ·019	− ·009	+ ·004	+ ·001	− ·007	+ ·001	− ·011	− ·019
1850...	− ·025	+ ·008	+ ·015	+ ·024	+ ·006	− ·004	+ ·006	+ ·010	+ ·002	− ·015	− ·007	+ ·007
1851...	+ ·021	+ ·011	+ ·002	− ·004	− ·009	− ·019	− ·020	+ ·001	+ ·005	+ ·004	− ·014	− ·001
1852...	+ ·002	+ ·007	− ·007	− ·006	− ·007	− ·001	+ ·004	+ ·008	+ ·001	+ ·035	− ·002	− ·010
1853...	− ·013	− ·020	− ·007	− ·003	+ ·004	− ·001	+ ·016	+ ·018	+ ·014	+ ·005	− ·015	− ·007
1854...	+ ·001	+ ·013	+ ·007	+ ·008	+ ·006	+ ·005	− ·007	− ·022	− ·038	− ·026	− ·006	+ ·007
1855...	+ ·006	+ ·004	+ ·004	+ ·011	+ ·010	+ ·007	+ ·006	+ ·004	+ ·006	− ·001	+ ·014	+ ·019
1856...	+ ·027	+ ·009	− ·012	− ·020	− ·026	− ·019	− ·018	+ ·006	+ ·017	+ ·021	+ ·011	+ ·002
1857...	− ·006	− ·018	− ·020	− ·014	− ·009	− ·014	− ·010	− ·005	+ ·016	+ ·027	+ ·024	+ ·020
1858...	+ ·005	+ ·012	− ·015	− ·020	− ·042	+ ·003	+ ·013	+ ·011	− ·002	− ·002	+ ·006	+ ·001
1859...	+ ·001	·000	+ ·004	+ ·006	+ ·009	− ·001	− ·008	+ ·013	+ ·004	+ ·003	− ·014	− ·009
1860...	− ·009	− ·006	− ·002	+ ·015	+ ·019	+ ·008	− ·013	− ·024	− ·018	− ·005	+ ·006	− ·001
1861...	− ·003	− ·003	− ·006	− ·013	− ·014	− ·004	+ ·002	+ ·005	+ ·007	+ ·015	+ ·004	− ·003
1862...	− ·011	− ·008	+ ·010	+ ·023	+ ·032	+ ·017	− ·001	− ·018	− ·033	− ·028	− ·024	− ·008
1863...	+ ·012	+ ·008	+ ·004	− ·017	− ·013	− ·014	− ·002	− ·004	− ·001	− ·007	− ·006	− ·001
1864...	+ ·004	+ ·005	+ ·013	+ ·018	+ ·023	+ ·007	− ·001	·000	+ ·009	+ ·009	+ ·002	− ·005
1865...	+ ·003	+ ·001	+ ·001	− ·007	− ·003	− ·006	− ·009	− ·017	− ·002	·000	− ·003	− ·011
1866...	− ·002	− ·001	+ ·004	+ ·006	+ ·012	+ ·006	+ ·002	− ·010	− ·013	− ·015	+ ·021	+ ·009
1867...	+ ·011	+ ·010	+ ·002	− ·005	− ·008	− ·002	− ·010	− ·031	− ·029	+ ·001	+ ·030	+ ·022
1868...	·000	− ·015	− ·007	+ ·004	+ ·008	+ ·003	+ ·010	+ ·007	+ ·010	+ ·001	− ·001	+ ·002
1869...	+ ·005	+ ·008	+ ·008	+ ·007	+ ·010	+ ·009	+ ·003	− ·011	− ·015	− ·004	− ·003	− ·015
1870...	− ·032	− ·028	− ·009	+ ·012	+ ·014	+ ·015	+ ·008	+ ·005	− ·001	·000	− ·003	− ·007
1871...	− ·017	− ·013	− ·002	+ ·003	+ ·008	+ ·006	+ ·019	+ ·019	+ ·008	− ·007	− ·015	− ·008
1872...	·000	+ ·001	− ·002	− ·004	+ ·010	+ ·007	+ ·006	− ·007	− ·004	− ·010	− ·016	− ·021
1873...	− ·012	+ ·001	+ ·007	+ ·001	− ·010	− ·007	− ·001	+ ·011	+ ·011	+ ·007	+ ·014	+ ·007
1874...	+ ·021	+ ·018	+ ·008	− ·003	− ·019	− ·013	− ·006	− ·005	− ·014	− ·010	+ ·005	+ ·013
1875...	+ ·002	− ·007	− ·009	− ·003	+ ·005	+ ·008	+ ·007	− ·002	− ·003	+ ·006	+ ·009	·000
1876...	− ·002	− ·012	− ·014	− ·015	− ·007	− ·003	− ·008	+ ·004	+ ·008	+ ·010	− ·006	− ·005
1877 ..	− ·002	+ ·001	− ·006	− ·005	+ ·018	+ ·030						

Table VII.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1850...	...	...	+·009	+·010	−·008	−·013	+·002	+·011	+·006	−·011	−·007	+·001
1851...	+·009	+·001	−·002	·000	+·002	−·006	−·008	+·008	+·007	+·004	−·014	−·002
1852...	+·003	+·009	−·003	−·002	−·001	·000	−·001	−·002	−·008	+·024	−·006	−·006
1853...	−·003	−·004	+·007	+·002	+·002	−·010	+·002	+·005	+·006	+·001	−·012	−·001
1854...	+·002	+·009	−·004	−·004	−·002	+·007	+·005	·000	−·013	−·004	+·006	+·008
1855...	+·001	−·004	−·005	+·001	+·001	+·001	+·002	+·001	·000	−·010	·000	·000
1856...	+·013	+·002	−·006	−·002	−·002	+·003	−·006	+·003	+·002	+·004	·000	−·002
1857...	−·001	−·005	·000	+·004	+·007	−·001	−·005	−·008	+·003	+·005	+·001	−·001
1858...	−·005	+·012	−·002	−·001	−·022	+·011	+·012	+·005	−·007	−·007	+·003	+·002
1859...	·000	−·002	+·001	+·002	+·004	−·002	−·012	+·009	+·001	+·006	−·005	·000
1860...	·000	−·004	−·006	+·005	+·011	+·006	−·005	−·011	−·002	+·002	+·009	·000
1861...	−·003	+·001	+·001	−·001	−·003	+·001	+·001	−·002	−·003	+·006	+·002	+·005
1862...	−·006	−·009	−·001	+·003	+·011	+·001	−·001	−·001	−·006	·000	−·004	·000
1863...	+·011	+·003	+·003	−·010	−·003	−·002	+·006	+·004	+·001	−·002	−·004	+·002
1864...	·000	−·003	·000	+·003	−·008	−·003	−·008	−·003	+·005	+·006	−·001	−·007
1865...	+·003	+·001	+·003	−·004	+·003	+·001	−·001	−·008	+·004	+·004	+·002	−·005
1866...	·000	−·002	·000	−·002	+·003	+·004	+·003	−·003	−·005	−·010	+·015	+·001
1867...	·000	+·001	−·006	−·004	·000	+·012	+·009	−·013	−·018	·000	+·021	+·012
1868...	−·006	−·015	−·004	+·007	+·007	−·002	·000	−·002	+·004	−·002	−·002	·000
1869...	+·002	+·001	−·001	−·002	+·002	+·004	+·001	−·007	−·008	+·006	+·013	+·003
1870...	−·010	−·010	·000	+·010	+·005	+·001	−·005	−·002	−·002	+·003	+·005	−·002
1871...	−·005	−·002	+·004	+·002	·000	−·007	+·003	+·007	+·003	−·005	−·009	+·001
1872...	+·006	+·004	−·003	−·007	+·004	+·002	+·003	−·004	+·002	+·002	·000	−·007
1873...	−·003	+·005	+·008	+·003	−·009	−·005	−·001	+·008	+·003	−·004	−·001	−·008
1874...	+·005	+·006	+·002	−·002	−·010	·000	+·007	+·004	−·007	−·008	+·004	+·010
1875...	+·003	−·009	−·007	−·001	+·005	+·006	+·003	−·006	−·006	+·002		

Table VIII.

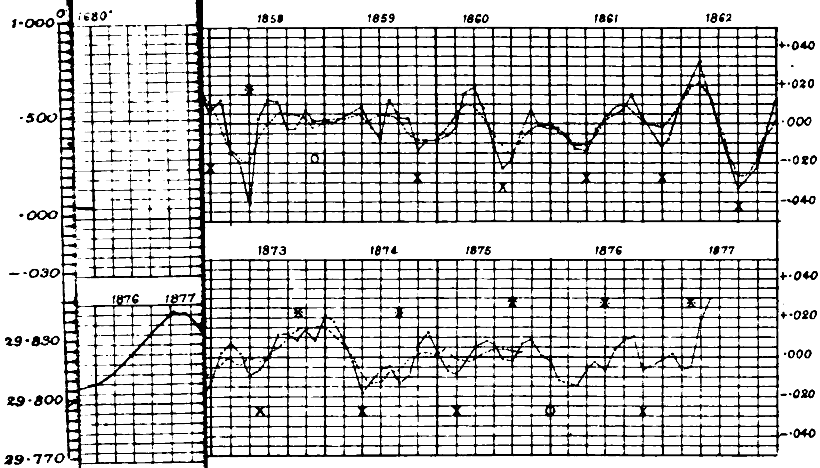
Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1850...	...	...	+·006	+·014	+·014	+·009	+·004	−·001	−·004	−·004	·000	+·006
1851...	+·012	+·010	+·004	−·004	−·011	−·013	−·012	−·007	−·002	·000	·000	+·001
1852...	−·001	−·002	−·004	−·004	−·006	−·001	+·005	+·010	+·009	+·011	+·004	−·004
1853...	−·010	−·016	−·014	−·005	+·002	+·009	+·014	+·013	+·008	+·004	−·003	−·006
1854...	−·001	+·004	+·011	+·012	+·008	−·002	−·012	−·022	−·025	−·022	−·012	−·001
1855...	+·005	+·008	+·009	+·010	+·009	+·006	+·004	+·003	+·006	+·009	+·014	+·019
1856...	+·014	+·007	−·006	−·018	−·024	−·022	−·012	+·003	+·015	+·017	+·011	+·004
1857...	−·005	−·013	−·020	−·018	−·016	−·013	−·005	+·003	+·013	+·022	+·023	+·021
1858...	+·010	·000	−·013	−·019	−·020	−·008	+·001	+·006	+·005	+·005	+·003	−·001
1859...	+·001	+·002	+·003	+·004	+·005	+·001	+·004	+·004	+·003	−·003	−·009	−·009
1860...	−·009	−·002	+·004	+·010	+·008	+·002	−·008	−·013	−·016	−·007	−·003	−·001
1861...	·000	−·004	−·007	−·012	−·011	−·005	+·001	+·007	+·010	+·009	+·002	−·002
1862...	−·003	+·001	+·011	+·020	+·021	+·016	·000	−·017	−·027	−·028	−·020	−·008
1863...	+·001	+·005	+·001	−·007	−·010	−·012	−·008	−·008	−·002	−·005	−·002	−·003
1864...	+·004	+·008	+·013	+·015	+·015	+·010	+·007	+·003	+·004	+·003	+·003	+·002
1865...	·000	·000	−·002	−·003	−·006	−·007	−·008	−·009	−·006	−·004	−·005	−·006
1866...	−·002	+·001	+·004	+·008	+·009	+·002	−·001	−·007	−·008	−·005	+·006	+·008
1867...	+·011	+·009	+·008	−·001	−·008	−·014	−·019	−·018	−·011	+·001	+·009	+·010
1868...	+·006	·000	−·003	−·003	+·001	+·005	+·019	+·009	+·006	+·003	+·001	+·002
1869...	+·003	+·007	+·009	+·009	+·008	+·005	+·002	−·004	−·007	−·010	−·016	−·018
1870...	−·022	−·018	−·009	+·002	+·009	+·014	+·013	+·007	+·001	−·003	−·008	−·005
1871...	−·012	−·011	−·006	+·001	+·008	+·013	+·016	+·012	+·005	−·002	−·006	−·009
1872...	−·006	−·003	+·001	+·003	+·006	+·005	+·003	−·003	−·006	−·012	−·016	−·014
1873...	−·009	−·004	−·001	−·002	−·001	−·002	·000	+·003	+·008	+·011	+·015	+·015
1874...	+·016	+·012	+·006	−·001	−·009	−·013	−·013	−·009	−·007	−·002	+·001	+·003
1875...	−·001	+·002	−·002	−·002	·000	+·002	+·004	+·004	+·003	+·004		

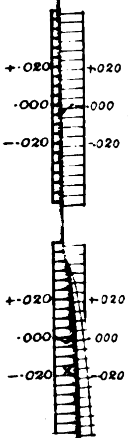
Table IX.

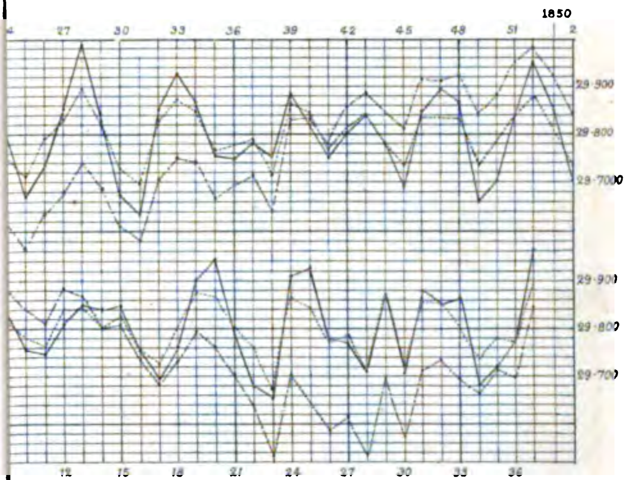
Week.	1848.	1849.	1850.	1851.	Week.	1848.	1849.	1850.	1851.
1..	29·931	29·955	29·924	26·965	27..	29·736	29·678	29·720	29·613
2..	·923	·930	·849	·934	28..	·600	·740	·649	·536
3..	·918	·926	·843	·982	29..	·620	·688	·583	·697
4..	·923	·962	·878	·917	30..	·650	·609	·557	·571
5..	·965	·948	·920	·942	31..	·692	·583	·735	·713
6..	·969	·975	·969	·877	32..	·693	·704	·697	·737
7..	·917	·904	·929	·915	33..	·693	·748	·748	·695
8..	·885	·830	·906	·868	34..	·722	·740	·752	·663
9..	·818	·911	·901	·885	35..	·756	·665	·747	·724
10..	·800	·879	·883	847	36..	·778	·695	·778	·714
11..	·865	·937	·879	·811	37..	·834	·715	·742	·846
12..	·838	·853	·884	·882	38..	·855	·642	·811	·836
13..	·826	·781	·902	·864	39..	·781	·832	·801	·753
14..	·803	·788	·852	·800	40..	·712	·838	·816	·762
15..	·846	·765	·795	·810	41..	·851	·781	·806	·834
16..	·721	·755	·829	·731	42..	·882	·859	·755	·817
17..	·704	·795	·801	·683	43..	·853	·889	·752	·937
18..	·777	·780	·792	·730	44..	·825	·846	·817	·802
19..	·759	·722	·789	·795	45..	·871	·817	·846	·829
20..	·754	·704	·758	·761	46..	·881	·915	·818	·804
21..	·706	·703	·764	·704	47..	·962	·911	·942	·902
22..	·693	·668	·728	·645	48..	·981	·925	·961	·926
23..	·651	·662	·667	·533	49..	·901	·848	·928	·975
24..	·571	·625	·592	·703	50..	·899	·885	·933	·929
25..	·618	·565	·646	·652	51..	·926	·950	·981	·966
26..	·750	·639	·650	·587	52..	·953	·976	·962	·930
					53..	..	..	·936	

Table X.

Week.	1848.	1849.	1850.	1851.	Week.	1848.	1849.	1850.	1851.
1..	..	+·019	+·023	+·004	27..	+·087	+·029	+·092	−·010
2..	..	−·013	−·057	+·029	28..	−·067	+·097	·000	−·088
3..	..	−·024	−·065	+·039	29..	−·057	+·025	−·073	+·061
4..	−·012	+·019	−·024	−·023	30..	−·019	−·070	−·113	−·081
5..	+·036	+·023	+·021	+·016	31..	+·025	−·104	+·061	+·054
6..	+·055	+·053	+·062	−·035	32..	+·004	+·027	+·009	+·051
7..	+·020	−·012	+·017	+·022	33..	−·019	+·070	+·032	+·007
8..	−·003	−·082	−·006	−·010	34..	−·016	+·047	+·009	−·064
9..	−·052	+·018	−·006	+·016	35..	−·006	−·036	−·007	−·021
10..	−·050	+·008	−·015	−·020	36..	+·004	−·025	+·010	−·033
11..	+·031	+·083	−·008	−·040	37..	+·057	−·017	−·036	+·089
12..	+·010	+·008	+·013	+·039	38..	+·060	−·096	+·025	+·055
13..	+·012	−·042	+·041	+·043	39..	−·032	+·066	+·014	−·042
14..	+·003	−·023	+·003	+·003	40..	−·112	+·044	+·033	−·064
15..	+·058	−·023	−·041	+·024	41..	+·028	−·031	+·012	+·014
16..	−·056	−·014	+·006	−·042	42..	+·057	+·022	−·044	−·002
17..	−·062	+·037	−·001	−·076	43..	+·014	+·040	−·049	+·111
18..	+·025	+·034	+·002	−·015	44..	−·050	−·014	−·002	−·044
19..	+·028	−·010	+·009	+·074	45..	−·023	−·063	+·004	−·031
20..	+·033	−·015	+·001	+·068	46..	−·015	+·036	−·048	−·078
21..	+·004	+·008	+·037	+·008	47..	+·059	+·033	+·050	+·021
22..	+·014	+·004	+·022	−·040	48..	+·064	+·032	+·045	+·022
23..	−·027	+·010	−·019	−·122	49..	−·028	−·068	−·004	+·056
24..	−·104	−·024	−·089	+·069	50..	−·041	−·032	−·016	
25..	−·042	−·089	−·019	+·042	51..	−·009	+·042	+·029	
26..	+·101	−·018	+·006	−·030	52..	+·026	+·080	+·006	
					53..	..	..	−·027	







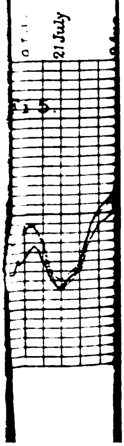


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Table XI.

Week.	1848.	1849.	1850.	1851.	Week.	1848.	1849.	1850.	1851.
1..	..	+·052	+·049	−·014	27..	+·143	+·051	+·191	−·024
2..	..	−·035	−·084	+·023	28.	−·108	+·185	−·003	−·092
3..	..	−·087	−·106	+·060	29..	−·120	+·036	−·105	+·072
4..	..	−·009	−·012	−·040	30..	−·016	−·129	−·151	−·097
5..	..	+·067	+·061	−·004	31..	+·106	−·171	+·101	+·080
6..	..	+·085	+·071	−·055	32..	+·053	+·052	+·058	+·051
7..	..	−·015	−·003	+·045	33..	−·013	+·130	+·034	+·062
8..	..	−·134	−·032	+·021	34..	−·060	+·064	−·035	−·120
9..	..	−·020	−·024	+·037	35..	−·056	−·047	−·017	−·073
10..	..	+·033	−·020	−·047	36..	−·006	−·056	+·008	−·031
11..	..	+·118	−·009	−·058	37..	+·113	−·020	−·044	+·162
12..	..	−·004	+·030	+·004	38..	+·080	−·046	+·008	
13..	..	−·051	+·047	+·046	39..	−·074	+·088	+·012	
14..	..	−·047	−·006	+·039	40..	−·175	+·027	+·054	
15..	..	−·012	−·057	+·046	41..	+·044	−·049	+·004	
16..	−·055	+·011	+·004	−·050	42..	+·137	+·006	−·049	
17..	−·073	+·055	+·029	−·112	43..	+·067	+·043	−·020	
18..	+·011	+·033	+·005	−·042	44..	−·103	−·018	+·025	
19..	+·031	−·037	−·034	+·101	45..	−·094	−·112	+·017	
20..	+·019	−·064	−·034	+·142	46..	−·016	+·048	−·052	
21..	+·018	−·011	+·064	−·015	47..	+·112	+·098	+·043	
22..	+·062	+·061	+·068	−·120	48..	+·084	+·065	+·027	
23..	−·033	+·073	+·006	−·143	49..	−·060	−·140	−·020	
24..	−·154	−·001	−·148	+·106	50..	−·099	−·100	−·039	
25..	−·043	−·136	−·049	+·127	51..	−·014	+·042	+·036	
26..	+·165	−·065	+·040	−·021	52..	+·086	+·148	+·019	
					53..	..	..	−·046	

March 6, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates for election into the Society were read from the Chair, as follows:—

Allman, Professor George Johnston, LL.D.
Atkinson, Professor Edmund, Ph.D.
Bagot, Alan, C.E.
Baird, A. W., Major R.E.
Balfour, Professor Isaac Bayley, D.Sc.
Baxendell, Joseph, F.R.A.S.
Bell, James, F.I.C.
Bidwell, Shelford, M.A.
Blake, Rev. Professor J. F., M.A.
Browne, Walter Raleigh, M.A.
Burdett, Henry Charles, F.L.S.
Buzzard, Thomas, M.D.
Claudet, Frederic, F.C.S.
Carpenter, Philip Herbert, D.Sc.
Colenso, William, F.L.S.
Conroy, Sir John, Bart., M.A., F.C.S.
Creak, Ettrick William, Staff Commander R.N.
Cunningham, Allan Joseph Champneys, Major R.E.
Curtis, Arthur Hill, D.Sc.
Forbes, Professor George, M.A.
Goodeve, Professor Thomas Minchin, M.A.
Green, Professor A. H., M.A.
Hartley, Professor Walter Noel, F.R.S.E.
Herschel, Professor Alexander Stewart, M.A.
Hicks, Henry, M.D.
Hicks, Professor William M., M.A.
Hudleston, Wilfrid H., M.A.
Japp, F. R., Ph.D.
Kent, William Saville.
Laughton, John Knox, M.A.
Lamb, Professor Horace, M.A.
Lewis, J. R., M.B.
Lyster, George Fosbery, M.I.C.E.
MacGillivray, Paul Howard, M.A.
McKendrick, Professor John G., M.D.
Manson, Patrick, M.D.
Marshall, Professor A. Milnes, M.D.
Meldola, Raphael, F.R.A.S.
Miller, Francis Bowyer, F.C.S.
Milne, Professor John, F.G.S.
Nobel, Alfred.
Ord, William Miller, M.D.
O'Sullivan, Cornelius.
Pattison, Samuel Rowles, F.G.S.
Perry, Professor John.
Pritchard, Urban, M.D.
Pye-Smith, Philip H., M.D.
Ransome, Arthur, M.D.
Rawlinson, Sir Robert, C.B., M.I.C.E.
Rendel, George Wightwick.

Rodwell, George F., F.R.A.S.
Roy, Prof. Charles Smart, M.D.
Rücker, Professor Arthur William, M.A.
Smith, Willoughby.
Spiller, John, F.C.S.
Stotherd, Richard Hugh, Colonel R.E.
Tate, Professor Ralph, F.G.S.
Tenison-Woods, Rev. Julian E., M.A.
Thomson, Joseph John, M.A.
Tidy, Charles Meymott, M.B.
Tonge, Morris, M.D.
Topley, William, F.G.S.
Tribe, Alfred, F.C.S.
Vivian, Sir H. Hussey, Bart.
Warren, Sir Charles, C.M.G., Lieutenant-Colonel R.E.
Warrington, Robert, F.C.S.
Watson, Professor Morrison, M.D.

The following Papers were read:—

I. "Magnetic Polarity and Neutrality." By Professor D. E. Hughes, F.R.S. Received February 23, 1884.

In recent papers upon the Theory of Magnetism,* I gave the opinion drawn from a long series of personal researches, that magnetism in iron and steel is entirely due to the inherent polarity of its molecules, the force of which could neither be destroyed nor augmented; that, when we have evident magnetism, the molecules rotate so as to have all their similar polarities in one direction; and that neutrality is a symmetrical arrangement or a balancing of polar forces, as in a closed circuit of mutual attractions. The series of researches which I now present bear unmistakable testimony to the truth of these views, showing the opposite polarities which exist in an apparently neutral bar of iron; and that it is by this means alone that external neutrality occurs in the iron cores of an electro-magnet upon the cessation of the inducing current.

The instrument used for measurements† consists of a delicate silk fibre-suspended magnetic needle, always brought to its zero-mark by the influence of a large magnet at a distance, the angle of which gives the degree of force required to balance any magnetised body placed on the opposite side of the needle. It can also employ electro-magnetic effects by the use of two opposing coils on each side of the needle, balanced so that an electric current passing through the coils has no influence on the needle, except when a piece of iron or steel is placed inside one of the coils; this again being balanced and measured by the large revolving magnet.

* "Proc. Roy. Soc." (vol. 35, p. 178), and "Journal of the Society of Telegraph Engineers," vol. xii, 1883.

† "On a Magnetic Balance, and Researches made therewith," by Professor D. E. Hughes, "Proc. Roy. Soc." (vol. 36, p. 167).

Before commencing my researches upon neutrality, I felt that it was necessary to observe the curves of magnetic penetration, whilst under the influence of its inducing cause. It is well known, however, from the researches of Gaugain, Du Moncel, and Jamin, that the magnetism does not penetrate to a very great depth with its full force, decreasing rapidly from the exterior to the interior. Most observations have been made by means of tubes of various thickness, introduced into each other. These, however, introduce an element of error, as, in separating them, they are necessarily drawn over each other. Jamin's method of dissolving the exterior of a steel magnet in diluted sulphuric acid gave results free from experimental error, but this could only be employed after the cessation of the inducing cause, the observations being really upon the permanent remaining magnetism.

The methods employed by myself consisted, first, in superposing twenty flat iron strips, $\frac{1}{2}$ millim. thick, 20 centims. long, and 3 centims. in width. These could be built up into a solid rod 1 centim. total thickness. Each piece was carefully selected and measured for its magnetic capacity, so that they should all be equal in value whilst under the influence of an inducing force, as well as their remaining magnetism when the influence ceased; the remaining magnetism being about $\frac{1}{4}$ of its capacity in the size and kind of iron employed.

These strips forming a compound bar were placed in contact with the poles of a strong permanent magnet, or they could be laid on one pole, the object being to polarise the lower bar only by contact, and observe the degree of penetration. The upper strip was carefully separated whilst the remainder was left under the polarising influence. We could thus separate each bar while under the influence without fear of reactions taking place between the separated bar and its companions. We had thus a bar or strip, separated while under the inducing influence, and, knowing its coefficient of remaining magnetism, we could estimate its full power when under the polarising influence. By this means the values were plotted graphically, giving curves of varying degrees, as the inducing force was changed, or the material of the strip from soft iron to hard steel. These curves were verified by a somewhat similar method, using a separate strip whose coefficient of remaining magnetism was known, and drawing this over the poles of a magnet, but separated from it by different degrees of thickness of iron.

These again were verified by an electro-magnetic method, in which a series of concentric tubes divided lengthwise was employed, so as to allow separation without friction, confirming the numerous curves obtained by the previous methods, showing that with a limited magnetising power acting upon homogeneous iron or steel, the penetration is inversely as the square of the distance from the inducing power, but

with high powers the exterior soon arrives at its saturation, the distant layers rise in value, and also if the bar is not homogeneous there is a consequent deformation, owing to the comparative rigidity of its molecules.

. In all cases, whatever the force employed, or nature of the iron or steel, there are no reversals of polarity in the interior, but a constant diminishing curve of penetration from the outside to the centre. This changes, however, the instant the exterior polarising force ceases, the different degrees of force between the external and internal react upon each other, producing the following results :—

Internal Waves of Opposite Polarity.

All varieties of iron and steel have a high magnetic capacity whilst under the influence of its inducing force, such as the electro-magnetic coils, or strong permanent magnets, but this power in a great measure disappears on the cessation of the inducing influence, a return more or less perfect towards neutrality being the result; remaining magnetism is therefore a partial neutrality, more perfect in soft iron, where the molecules are in a greater state of freedom, than in comparatively rigid cast steel. Our so-called permanent magnets are simply the remains of a far higher magnetic state, and it is already in most cases half-way down on its road to neutrality.

It is absolutely necessary in a theory of magnetism that we should know the cause of neutrality, for it is really the starting point to appreciate how polarity becomes evident. In my previous researches upon neutrality I used the induction balance, but in these I have employed more simple methods, which allow of repetition by the most simple means.

The first consists in forming compound bars of ordinary hoop-iron, $\frac{1}{2}$ millim. thick, and 30 or more centims. long, twenty or more of which could be superposed, bound together by a fine copper wire and forming a rod of any desired thickness; they were magnetised by drawing over magnets of various powers, and the degree of approach to neutrality observed by the amount of its remaining magnetism. Now, on carefully separating them, there were invariably found violent curves of opposing magnetism, previously held bound by the closed circuit of mutual attractions.

The second method consists in superposing the divided concentric tubes, already mentioned, bound together by a fine copper wire, and magnetising them in the electro-magnetic coils of the measuring balance; by this means we could observe the charge or full magnetic capacity under the influence of an electric current, the remaining magnetism upon its cessation, and after taking out the tubular core, separate it, and observe the polarity of its successive internal layers. This method is objectionable, as the slightest rubbing of one tubular

surface against another may alter the true value. The electro-magnetic method is however infinitely superior when observations are made on solid bars, or tubes of different degrees of thickness, to observe the influence of depth or thickness, in producing a perfect return to neutrality after cessation of the inducing effect of the coils.

The third method was a chemical one, somewhat similar to that employed by Jamin, except that as the object was to study the curves of neutrality, the bars were of annealed steel, highly magnetised in the coils, and afterwards reduced almost to a zero, by vibrating them, or beating them gently with a wooden mallet. We had by this means aided the molecules to follow their inclination, as they do in soft iron, for when a soft steel rod is in a state of vibration, its molecules are comparatively free; but they rigidly retain the true curve of neutrality when not vibrated. We are thus enabled by dissolving the exterior in various dilute acids, and by taking repeated observations, to draw graphically the waves of opposing polarities, which have produced external neutrality.

The curves obtained by the different methods are identical in form. The simplest and most accurate method is the first, as we can choose a hard variety of iron, such as ordinary hoop-iron, and by slight vibrations, or blows with a mallet, allow the molecules sufficient freedom to form their curve before separating, and as the material is sufficiently rigid not to be influenced by mere contact, or even frictional drawings, we have on each strip a perfect record of its state, and can thus analyse the internal state of a neutral compound bar.

If we take a compound bar of the hoop-iron, and draw the lower side over the south pole of a magnet, it will be found nearly neutral, or if not sufficiently so, we can reduce it by slight blows with a mallet: suppose the united bar gives still a remaining magnetism of 18° on the magnetic balance, on separating the components and observing the same ends we find the lowest (or the bar which had touched the magnet) 150° north polarity, the next may be slightly north or zero; the rest will have varying degrees of south polarity, from 60° to 10°, the total of which exactly balances the north polarity of 150, less 18°, which we already observed as the remaining magnetism.

If we do not wish to approach a perfect neutrality, we should not vibrate the rods. In this case we may have 75° of remaining magnetism, and find on separating the strips, that we have on lower strip 150° north, and the total opposing south polarity of the interior but 75° south, leaving the remaining 75° of north polarity first observed on the compound bar unbalanced.

The mutual reactions between the magnetic molecules in a solid bar are precisely similar to those between two or more separate bars,

the reactions in the solid bar being more pronounced and complete than those obtained through a separation of air; the greater the separation the less the reaction, but in no case will the law of neutrality be changed.

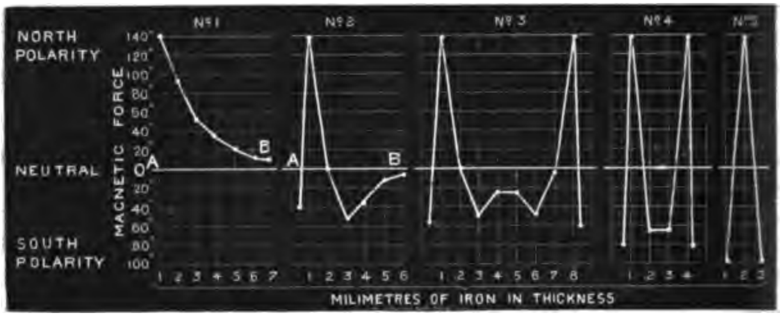
In homogeneous iron or steel, we have a well-defined curve, the distance of which can be calculated from observation upon the remaining magnetism, but if the interior is harder than the exterior, the inner portion will from its rigidity preserve its previous magnetism, reversing entirely the outer portions. This occurs also in small electro-magnets where from the small number of molecules in the interior compared with the vastly greater exterior, and also all the surrounding inducing lines of magnetic force acting on the centre from all sides, the outside is completely reversed to a remarkable depth.

I have been enabled to prove this by the chemical method, employing either dilute sulphuric acid, acidulated bichromate of potass, or dilute nitric acid (1 pint acid to 5 water), the latter being far more rapid and equal in its action. The following experiment will show rapidly the influence of the outside reversal polarity. Let us take a soft steel wire 1 millim. diameter, 10 centims. long. Magnetise it in a coil, or by drawing over a strong permanent magnet, so that it has perhaps a remaining magnetism of 200°. If we vibrate this rod or give several blows from a mallet, we can reduce this to 25°; we have now almost perfect neutrality, having only a remaining magnetism of 25°, which remains a constant for years if not remagnetised. Place this rod in dilute nitric acid, and in fifteen minutes it will rise to 50°, or double its previous value, in one hour to 75°, and two or three hours to 100°, or four times its previous force; the increased force of 75° has been rendered evident by dissolving an equal opposing polarity of 75°, so that we have already found $75+75+25=175°$, or 87 per cent. of its highest force. This is so easily repeated with soft steels of all sizes and dimensions, that there can no longer be any doubt as to the existence of the outside reversed polarity. The experiment is more difficult to repeat with soft iron, as from the freedom of its molecules a fresh outside reversed curve is formed anew as the exterior is dissolved, the balancing curves reproducing themselves until we have almost entirely dissolved the iron; still with care, and iron not too soft, we can render evident all the neutral curves seen in steel.*

The curves obtained by the various methods are so numerous, each requiring more space than the limits of this paper will allow, that I

* Thin flat steel, such as clock-springs, saw-blades, or ribbon steel, well annealed, are most suitable for this experiment. They may be of any width or length; the thickness may vary between $\frac{1}{4}$ and $\frac{3}{4}$ millim. I have found that strips of $\frac{1}{2}$ millim. thick give the highest result.

am forced to give roughly the general outlines, as in fig. 1. Suppose we take a compound bar of iron, of eleven strips, and draw it over a permanent magnet, polarising its lower side only, its neutrality may be found nearly perfect, or 15° of remaining north polarity at the north end; now on carefully separating these rods and observing the same ends, we have for the lower or the side which had been



magnetised, 350° north; the following successively observed would give 10° south, 35° S., 55° S., 60° S., 50° S., 40° S., 22° S., 10° S., 6° S., 5° S.; here all the superposed bars are opposite in polarity to the exterior 350° north, the total south observed being 292, plus 30° south obtained from the exterior, by the coating of its lower face with the $\frac{1}{10}$ milim. of iron strip already mentioned. We are thus enabled to account for 323° degrees south, and 350° north, leaving a remaining magnetism unaccounted for of 12°, which was doubtless disseminated on the surface of each bar on separation.

The above curves were obtained from the same polar (north) end of a compound bar of iron, the south or opposite end of the bar would give reversed curves to these. The curves are reductions to a similar force, but do not exhibit the perfection of the curves obtained on a larger scale.

No. 1 represents the typical curve of penetration of a bar of iron, whilst under the influence of an exterior polarising force, applied at A, or only at one side of a bar. When the force is applied to the whole of the exterior surface (as in a coil), A would represent the polar force on its surface, whilst B the interior. In all cases there would be a depression at the centre; great if the bar is thick, and comparatively small if the bar is thin. The curve rises with the exterior polarisation force, but in no case can a reversal ensue whilst under its influence. The instant, however, that it ceases, the higher magnetic power of the exterior layers reacts gradually and successively upon the weaker interior layers, rotating them through neutrality to a strong opposite polarity.

This is seen in No. 2, which represents exactly what takes place in No. 1 upon the cessation of the inducing influence. We notice that the first portion of the exterior has rotated to south polarity, followed by an intense north, but not of great density; its reaction, being more violent, rapidly rotates all the interior to a south polarity, gradually weakening in intensity as the distance increases from the inducing north polarity. The exterior, in fact, reacts upon its interior precisely as before the inducing exterior magnet reacted upon the whole. In No. 1 the south pole of the permanent magnet produced a continuous curve of north the instant this ceased, the north of the exterior produced an interior south, and if these are perfectly balanced, then and then only will the bar become neutral.

When both sides of a bar are polarised at the same time, then we have two similar curves to No. 2, as shown at No. 3, the diminishing curves of internal opposing polarity overlapping each other; the curve represents those obtained on bars 2 centims. in thickness. If the inducing force is great the penetration is greater and more intense, reacting more violently, and the central depression of the opposing waves is less pronounced. If we keep the previous force and diminish the thickness of the bar, the two central waves cross each other, and at last, as in No. 4, we have only one wave; this occurs with bars of but 3 millims. thickness. We notice here that from a want of sufficient material in the centre of the iron, it is constrained to force its central wave to a far higher degree, and that the exterior now also commences to be reacted upon more violently. Evidently the conditions are strained, and we shall see the result later. This want of sufficient material to form the internal opposing wave of polarity is shown when we reduce the thickness of the bar to 1 millim., the width being 3 centims., and the length 30 or more, as in all previous cases. Here there are no traces of an internal curve, the opposing polarity, as shown in No. 5, being entirely on the surface.

I have shown that we may clearly perceive this curve by dissolving its exterior in dilute nitric acid, but as I employed vibrations to reduce it to neutrality, this might give rise to objections on the score of mechanical reactions. To meet this objection several strips of magnetised steel of various forms, but all $\frac{1}{2}$ millim. in thickness, were reduced almost to neutrality by simply heating them to a dull red heat, allowing them to cool slowly. These gave remarkable results, proving that the vibrations caused by heat are similar in results to mechanical vibrations, and I found that in most cases their external evident magnetism was increased 100 per cent. by an immersion of fifteen minutes, and 600 per cent. in one hour.

Interesting results can be obtained by this method, but if rapidity of chemical action is desired we must first remove the scale or oxide

on the exterior by polishing with emery paper, or dissolve this first in acidulated bichromate of potass.

A perfect curve of these opposing polarities can be obtained by placing a glass vessel containing the steel and solvent in the balance itself, taking continual observations during its solution, and we may thus observe the gradual rise in force to a maximum, then its fall to zero, to an opposing polarity, completely verifying all previous observations.

Supposing the magnetised steel previous to heating gave 200° we should reduce it to 50° if heated to dull red, a bright red heat would probably reduce it to 20°; we should then start from an almost perfect neutrality to find, on dissolving its exterior (and allowing for the reversed polarity of the reversed portion), all its previous polarity.

Faraday remarked that the magnetic qualities of iron disappeared at yellow-red heat (1050° C.), reappearing gradually when cooled to red heat (700° C.). I have found that if we heat the steel to yellow-red heat the whole previous structure disappears, and does not re-appear on cooling. No satisfactory explanation, as far as I am aware, has been offered relative to the disappearance of the magnetic qualities of iron and steel at certain temperatures, but noticing that its internal structure is also changed, the following hypothesis may explain the phenomenon.

Assuming that increased heat increases molecular vibration, and that molecules would oscillate to a degree dangerous for the stability of any previous structure, a moment would arrive when the oscillations were so great that all structural formations disappear; and precisely at this instant there would be no external evidence of polarity, or magnetic quality, as the molecules would be oscillating through a range on both sides of external neutrality. On cooling (the previous structure having disappeared) they would satisfy their mutual attractions by the shortest path, forming probably, if perfectly free, a closed circuit of two, grouping themselves as a double molecule; but if a directing influence, such as a continuous current of electricity, was passed through the bar, then they would obey this influence, and in the latter case the closed magnetic circuit would be in concentric circles, as I have demonstrated in previous papers.

A similar effect is caused by mechanical vibrations. I have already shown that we increase the internal curves by gentle blows of a mallet, thus allowing the molecules sufficient freedom to follow their path, as in the case of red heat; but if we strike violently upon the end of the rod, the whole structure is broken down by the violent oscillations of its molecules, and the neutrality now resembles exactly that produced at yellow-red heat.

The theory of symmetrical neutrality which I have demonstrated,

requires that there should be a sufficient thickness in a bar of iron or steel in order to produce a symmetrical opposing polarity. Coulomb's theory of the neutrality taking place in the molecule itself requires no thickness except that of a molecule. Ampère's theory could allow of heterogeneity on the surface as easily as in the interior, consequently thickness of a bar would, according to these theories, have no favourable result; but if the theory that I have advanced is true, thickness should have the greatest possible influence. An extremely thin strip or bar of iron should have an infinitely higher proportionate remaining magnetism from the want of interior reaction, whilst an extremely large solid bar should have infinitely less proportionate remaining magnetism. This at once allows us to test the truth of the theory by an independent method free from all experimental errors, as we may place in the coil of the magnetic balance bars of iron or steel of different degrees of thickness, observe their magnetic capacity whilst under the influence of an electric current, and the degree of remaining magnetism on its cessation, and note the extraordinary influence which thickness has in allowing space for the opposing waves of polarity to produce instantly a higher degree of neutrality than is possible without its aid.

The conditions of the experiments are really those of ordinary electro-magnets, the iron or steel under observation is simply at the time of observation a core of an electro-magnet.

Numerous experiments were made on this subject, all confirming the views advanced. A few examples will be sufficient to include them all, for if we place in the coil of the balance different thicknesses of the same diameter and length of iron or steel, we notice a marked rise in its exterior force or magnetic capacity while under the influence of the electric current, and upon its cessation an equally marked return to a more perfect neutrality with each increase of thickness. The table on the following page contains sufficient examples to show this clearly.

This table gives the results of round cores; experiments, however, were also made with flat bars with like results, the form or length having no direct influence, as the reactions are transversal and localised from a point in the exterior to one in the interior.

Comparing No. 1 of the table (consisting of an extremely thin sheet-iron tube) with No. 2 (a solid bar of iron of exactly similar size), we have for the thin tube a remaining magnetism of 50 per cent. of its previous polar force, and in the solid bar we have only 3 per cent.; whilst in the solid bar, where the opposing waves of polarity could easily form and produce a near approach to neutrality, we find that its polar force under the influence of the coil is 400 per cent. greater than that of the thin tube.

Although, as well known, hard steel has a higher retaining power,

No.				Magnetic capacity under influence of the coil. 1 Daniell element.	Remaining magnetism on cessation of the electric current.
1.	Tube of thin soft iron, 2 centims. diameter, 20 centims. long, $\frac{1}{10}$ millim. thickness			218°	106°
2.	Similar size solid rod of soft iron			960	29
3.	„ „ cast steel, tempered			458	18
4.	„ „ bundle of 1 millim. diameter soft iron wires			1268	142
5.	„ „ glass tube filled with iron filings			160	15
6.	Soft Swedish iron wire, 1 millim. diameter			455	105
7.	Hard tempered cast-steel wire, 1 millim. diameter			49	16
8.	Brass tube	Electro-plated with iron extremely thin	3 centims. diameter 20 „ long	0·95	0·94
9.	„	Electro-plated with iron to $\frac{1}{10}$ millim. thickness	„ „	231	109
10.	„	Ditto, 1 millim. thickness	„ „	401	72
11.	„	„ 1 centim. „	4 centims. diameter	1075	35

still, this can be reduced far below that of the soft thin iron if sufficient thickness is allowed in order to produce the internal reactions. This is shown in No. 3, where a solid 2 centims. diameter of hard-cast steel has double the force of the thin soft iron under polarising influence, and its remaining magnetism only 4 per cent. of its previous force. This shows clearly that Jamin's views of the superiority of thin steel bars over thick where permanent magnetism is desired, are fully confirmed, as in order to have raised the cast-steel 2 centims. thick bar to a high remaining magnetism, we should have had to employ at least fifty times stronger inducing force than that necessary for the thin bars. The proportion of remaining magnetism in iron or steel to the inducing force is almost similar throughout the entire range up to saturation, where the remaining magnetism is no longer proportional to the inducing power, but remains a constant, no matter how high and powerful the influence excited. The molecules have simply then rotated to parallelism and cannot rotate further without diminishing its force, and the sudden spring back to a partial neutrality is then the same for all forces above that of saturation. The proportion of remaining magnetism to that of its magnetic capacity under the influence of an inducing field, is shown in Nos. 6 and 7, where iron and steel wires of similar diameter have not a wide difference, the remaining magnetism here

being 25 per cent. for iron, and 33 per cent. for steel of its proportionate previous force.

The most conclusive experiments, however, will be seen in Nos. 8, 9, 10, and 11. No. 8 being a brass tube coated with an exceedingly thin transparent coat of iron (I was unable to measure its thickness), this thin coating of iron was easily raised to its saturation by a feeble battery, from which point no increase of battery power had the slightest effect, giving always 0°·95. The extraordinary effect of thinness was seen on taking off the inducing influence, no perceptible movement of the needle on the balance occurred, indicating that its retaining power was the same as its capacity or 100 per cent. of retention; vibrations and hammering which reduce a solid bar at once to perfect neutrality had no effect. I have, however, marked it down as 0°·94 as a limit of experimental error; if we assume only 80 per cent. of its previous force it is still sufficiently remarkable. At No. 9 where the brass was coated to a measurable thickness of $\frac{1}{10}$ millim., we already see a better return to neutrality, having now only 50 per cent. of proportionate remaining magnetism; at No. 10, where we have 1 millim. thickness, we have improved our neutrality by having only 20 per cent., and at No. 11 we have, by means of an increased thickness of 1 centim., almost completely allowed the balancing waves of opposite polarity perfect formation, the remaining magnetism now being only 3 per cent. of its proportional power; and if we even neglect the proportional power, we see that the 1 centim. bar has far less remaining magnetism than that of 1 or $\frac{1}{10}$ millim., whilst its magnetic capacity is far higher.

No. 4 shows that while bundles of wires have a higher remaining magnetism than solid (due to the want of homogeneity allowing perfect formation of the opposing waves), still from their increased surface exposed to the inducing effect they give a higher magnetic effect, the differential effect (as that employed in temporary electro-magnetic and induction coils) is greater, being here for a solid bar 960−29=931, and for the bundles of iron wire 1,268−142=1,126 useful effect.*

The effect of thickness even upon finely divided iron such as filings is shown in No. 5, where the remaining magnetism is only 9 per cent. against 50 per cent. as shown in No. 1, and we have precisely similar

* If we require a continuous magnetic effect, as in the field magnets of dynamo machines or small constant electro-magnets for extremely feeble electromotive force, solid cores or bundles of wire of large diameter should be employed, but as the time of charge and discharge increases with the diameter, it would be unsuitable for electro-magnets requiring rapid charges, such as those employed for telegraph relays, large electro-magnets requiring several seconds to charge them to saturation, while extremely small electro-magnets may be saturated in the $\frac{1}{1000}$ part of a second.

results with thin slices of iron filings as we do with the solid iron in sheets or tubes.

It would be difficult to explain these effects upon any hypothesis except that of molecular rotation. For, if we regard it as simply a case of magnetic induction, the stronger reacting upon the weaker, we fail to explain the perfect spiral form of the opposing waves, and above all, the reversal of the exterior, which was evidently the most strongly polarised, but if we suppose that the similar polarities of all the molecules have rotated, symmetrically pointing their north polarities to the evident north end of the bar, we have only to imagine a series of magnetic needles superposed with all their north polarities pointing in one direction brought and held there by the influence of a strong external magnet. If this influence was nearer the first needle than the last, we should have a slight spiral due to its diminishing effect, precisely as we notice in the curve in iron whilst under external influence. Now withdraw the exterior force, the needles would react against each other, and as they are free to move in all directions, there would be an increased spiral, the outside being reversed to its previous position, while the spiral would continue in the interior, reversing the larger portion of the needles until they all found a position of equilibrium, which would then represent neutrality.

If we had no frictional resistance to molecular rotation, we should obtain perfectly balanced curves in comparatively thin iron, but as this resistance is great and demonstrated by the loosening influence of mechanical vibrations, we require a certain depth of iron so that a complete curve shall be easily obtained with comparatively infinitely small motion of each molecule.

That inherent magnetic polarity is a quality of all matter, solid, liquid, gaseous, and the ether itself, varying only in degree and not in nature, seems demonstrated by a series of researches I have been making upon the mechanism employed in magnetic conduction through the atmosphere and Crookes's vacuum.

These researches are being made by means of the induction and magnetic balance. They prove that the atmosphere, and presumably the ether as well as all liquids and gases, have their saturation point similar to iron, that the curve is the same as in perfect soft iron, and that the highest magnetic capacity of iron does not exceed that of the atmosphere by more than forty times, consequently we may fairly assume that the ether may be regarded as an extremely magnetic body, obeying the same laws as those of iron; and as I regard the symmetrical rotation of magnetic molecules as the cause of evident magnetism in iron, and as the difference in force between iron, copper, and ether is simply a differential one, I believe that the neutrality which appears in all paramagnetic and diamagnetic bodies, wherever

the exciting influence is withdrawn, is formed by mutual molecular reactions producing closed circuits of mutual attractions as demonstrated in iron.

A line of force between a magnet and its armature is to me simply a line of molecular rotation, lines would neither be added nor subtracted, they could simply be rotated from a symmetrical neutrality to an equal symmetrical point of saturation.

In my paper upon the theory of magnetism, I showed that there were several molecular arrangements which produced external neutrality, the circular chain of molecules, when an electric current passes through an iron wire, a neutrality produced by an artificial superposition of a weaker contrary magnetism upon one more internal, and made the supposition that were it possible to have a piece of iron free from the influence of the earth, then (if there had been no previous magnetisation directing the structure) the molecules would short circuit their mutual attractions in the shortest path.

The experiments cited in this paper are of an extremely simple nature, and after being verified by independent observers can no longer leave doubt as to the cause of neutrality.

Whatever theory we adopt as an explanation of evident magnetism, it will be found that neutrality occurring after the cessation of an external inducing force upon a bar of iron or steel, is the result of symmetrically opposed polar forces, producing apparent waves of opposite polarity, or reactions between the exterior and interior of a bar of iron.

II. "On the Origin of the Fibrin Ferment." By L. C. WOOLDRIDGE, M.B., D.Sc., George Henry Lewes Student. Communicated by Professor M. FOSTER, Sec. R.S. Received February 26, 1884.

The "fibrin ferment" which makes its appearance in shed blood is generally, I believe, supposed to arise from the cellular elements of blood, either from ordinary white corpuscles or from some special kind of corpuscles, the cells so concerned discharging the ferment into the blood or setting it free by their actual disintegration. Without wishing to deny that this may be one source of fibrin ferment, I am able, I think, to bring forward evidence that ferment may make its appearance in blood-plasma perfectly free from cellular, and indeed from all formed elements, in which case it must arise from some constituents of the plasma itself, and not from cells of any kind.

It will be most convenient, perhaps, if I state the facts which I have to bring forward in connexion with two series of experiments.

I. A measured quantity of blood was received directly from the carotid of a dog into a vessel containing an equal bulk of a 10 per cent. solution of common salt, great care being taken that the complete admixture of the blood and salt solution was effected as rapidly as possible. By the help of the centrifugal machine plasma was separated from this "salted blood," and this plasma was again subjected to the action of the machine until all traces of formed elements were removed. As is well known, a portion of such a plasma diluted with five times its bulk of water coagulates rapidly, whereas the undiluted plasma remains liquid for an almost indefinite time.

According to commonly received opinions, such a "salted plasma" contains all the fibrin factors, including the ferment, the latter having already passed out of the cells into the plasma; and the reason given of the absence of coagulation in such a salted plasma and its occurrence upon dilution is, that the presence of the salts presents a hindrance to the action of the fibrin ferment, and that this obstructive influence of the salt is removed by the dilution of the mass.

No one, however, as far as I know, has taken the trouble to ascertain whether fibrin ferment is present in such salted plasma. And, as a matter of fact, it is not; whereas it does make its appearance as soon as dilution with water has taken place, as the following experiment shows:—

A portion of the undiluted salted plasma was treated with absolute alcohol in large excess, and the precipitate after being allowed to remain under the alcohol for three or four weeks was dried at a low temperature and extracted with water—that is to say, the plasma was treated in the way usually adopted for obtaining a solution of ferment fairly free from proteids, &c. A portion of the diluted plasma, or rather of the serum resulting from the coagulation of the diluted plasma, was treated in an exactly similar manner.

The aqueous extract of the diluted plasma brought about coagulation in specimens of magnesium sulphate plasma (such as is usually employed for testing the presence of fibrin ferment) in from ten to fifteen minutes. The aqueous extract of the undiluted plasma brought about no coagulation in specimens of the same magnesium sulphate plasma, even after the lapse of eighteen hours.

The conditions of each experiment were made as exactly alike as possible; and the conclusion seemed inevitable that ferment is present in the diluted and coagulated plasma, but absent from the undiluted plasma.

This conclusion is, moreover, supported by the following experiments:—To a portion of the undiluted plasma above mentioned a small quantity of fibrin ferment was added, in the form of the dried precipitate thrown down by alcohol, *i.e.*, a mixture of coagulated proteids and ferment. Coagulation took place. I have no record of

the exact time elapsing between the addition of ferment and the appearance of the clot, but it was certainly not longer than three or four hours.

II. Of the so-called peptone-plasma (*i.e.*, plasma of the blood of a dog after the injection of peptone into the veins, such blood, as is well known, coagulating with great difficulty), freed from all cellular elements by the centrifugal machine, two portions were taken.

To the one (A) a quantity of *lecithin* was added, the lecithin being rubbed up with the plasma so as to be diffused through it; the other (B) was left untouched.

Through both a stream of carbonic acid was passed, with the result that while A clotted in about ten minutes, B after the lapse of half an hour showed no disposition whatever to coagulate. Both portions were then treated with excess of alcohol for the extraction of fibrin ferment in the usual way. The aqueous extract of A proved to be exceedingly rich in ferment, producing coagulation in magnesium sulphate plasma in about ten minutes. The similarly prepared aqueous solution of B produced no coagulation at all.

Now I have elsewhere,* in discussing the action of lecithin in promoting coagulation, shown that the coagulation which is brought about by the addition of lecithin is not due to the lecithin or to any of its products of decomposition acting after the manner of a ferment, or to its carrying a fibrin ferment with it. In this case, therefore, as in the previous case of "salted" plasma, the ferment appears to be absent *before* coagulation, but to be present *after* coagulation.

I may here call attention to an observation made by Rauschenbach.† This observer found that the addition of yeast to plasma, prevented from coagulating by exposure to cold, brought about coagulation, and at the same time gave rise to the appearance of a large quantity of fibrin ferment. Nevertheless, he completely failed to extract any fibrin ferment from the yeast itself. Now yeast is very rich in lecithin, and it seems highly probable that the coagulation caused by yeast was due to the lecithin contained in it, and hence the appearance of the fibrin ferment after the addition of yeast, and consequent coagulation, is quite parallel to the result of the experiment with lecithin and peptone-plasma recorded above. In both cases the ferment appears to have arisen out of the plasma itself.

It is possible to obtain a coagulation in peptone-plasma without the addition of lecithin. For this purpose large dilution is necessary, followed by the passage of a stream of carbonic acid gas. But in such a case, however, coagulation is not only long in making its appearance, but the fibrin is formed, so to speak, in successive crops. Thus a feeble coagulation first appears, and if the clot so formed be removed, a

* "Journ. of Physiol.," vol. iv. p. 226.

† "Blutplasma u. Protoplasma," Inaug. Diss., Dorpat.

succeeding coagulation is observed some time later, to be followed in turn by a third, and so on. When lecithin, on the other hand, is added, without previous dilution, the clotting is speedy and complete.

If the serum thus resulting from the coagulation of peptone-plasma brought about by large dilution and treatment with carbonic acid, be examined for fibrin ferment in the usual way, it will be found to contain ferment, though much less than could be obtained from a corresponding quantity of the same plasma coagulated rapidly by the addition of lecithin. The relative amount of ferment appearing under different circumstances is illustrated by the following experiment:—

Of three equal portions of the same peptone-plasma, one portion was simply treated with a stream of carbonic acid gas, without any dilution, and did not coagulate; a second was treated with a stream of the same gas after large dilution, and coagulated slowly; to a third lecithin was added, and a stream of carbonic acid passed through it, with the result of producing a rapid and complete coagulation.

All three portions were treated in the same way for the extraction of the fibrin ferment, and the activity of the three aqueous extracts then prepared was tested under exactly the same conditions, with the help of magnesium sulphate plasma.

The first produced no coagulation after the lapse of twenty hours.

The second produced coagulation in four hours.

The third produced coagulation in five minutes.

The amount of ferment seems to be in proportion to the energy of coagulation and the presence of ferment after simple dilution, and the action of carbonic acid gas shows that the ferment appearing after coagulation by the help of lecithin does not come from the lecithin itself.

Thus there is a remarkable coincidence between the occurrence of coagulation itself and the appearance of the fibrin ferment, and that in plasma freed most carefully from all cellular elements.

I believe, therefore, that I am justified in concluding that though fibrin ferment does not pre-exist in normal plasma, it may make its appearance in that plasma in the absence of all cellular elements, and must therefore come from some constituent or constituents of the plasma itself.

I am still engaged in investigations directed to find out what that constituent is, or what those constituents are.

March 13, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Researches in Spectrum Photography in Relation to new Methods of Quantitative Chemical Analysis. Part II."* By W. N. HARTLEY, F.R.S.E., &c., Professor of Chemistry, Royal College of Science, Dublin. Communicated by Professor G. G. STOKES, Sec. R.S. Received February 28, 1884.

(Abstract.)

This paper includes an introduction recording the methods which have been proposed by different authors for the quantitative estimation of various metallic elements. An account is then given of the length and strength of metallic lines in solutions of definite strength. Under given conditions each solution emits a characteristic spectrum. In the case of magnesium, a minute description is given of the spectra presented by various solutions containing from 1 per cent. to 0·00000001 per cent. of the metal, but in the case of other elements tabular descriptions of the spectra of solutions containing 1, 0·1, and 0·01, in some instances 0·001, of metal are given together with carefully drawn maps. The substances thus treated of are magnesium, zinc, cadmium, aluminium, indium, thallium, copper, silver, mercury, tin, lead, tellurium, arsenic, antimony, and bismuth.

The sensitiveness of the spectrum reaction is practically unlimited when applied to magnesium compounds dissolved in water, since it was shown that with a given length of spark, $\frac{1}{10000000}$ of a milligram could easily be detected; when, however, the strength of spark was greatly increased, but the striking distance between the electrodes left unaltered, the sensitiveness was increased ten thousand-fold. In point

* For Part I see "Phil. Trans.," Part I, 1884.

of fact, one part of magnesium was detected in 10,000,000,000 parts of water, the lines seen under these circumstances being those with wave-lengths 2801·6 and 2794·1. The spectrum reaction of arsenic is the weakest, those of antimony and tellurium are also weak, while that of bismuth is not strong. In fact it is noticeable that the more strongly basic elements are those with the most persistent lines.

Evidence is afforded in the case of the aluminium spectrum that it is not invariably the longest or strongest line which is the most persistent. The line with wave-length 3584·4 is both longer and stronger than a pair of lines adjacent thereto with wave-lengths 3612·4 and 3601·1, but whereas the first is not seen in solutions containing 0·1 per cent. of aluminium, the pair are still visible in solutions containing 0·01 per cent. Under certain conditions this single line appears the longest in the whole spectrum, whereas otherwise, and under most circumstances, the lines with wave-lengths 3960·9 and 3943·4 are longest.

As a rule, even the longest lines are shortened by great dilution of the solutions, but there is a pair of lines in the spectrum of copper with wave-lengths 3273·2 and 3246·9 which become greatly attenuated, yet nevertheless remain long lines till they finally disappear.

It is shown by one or two examples how the tables of spectra and accompanying maps may be employed in rendering quantitative results. The special applications of this method it is proposed to describe in a further communication.

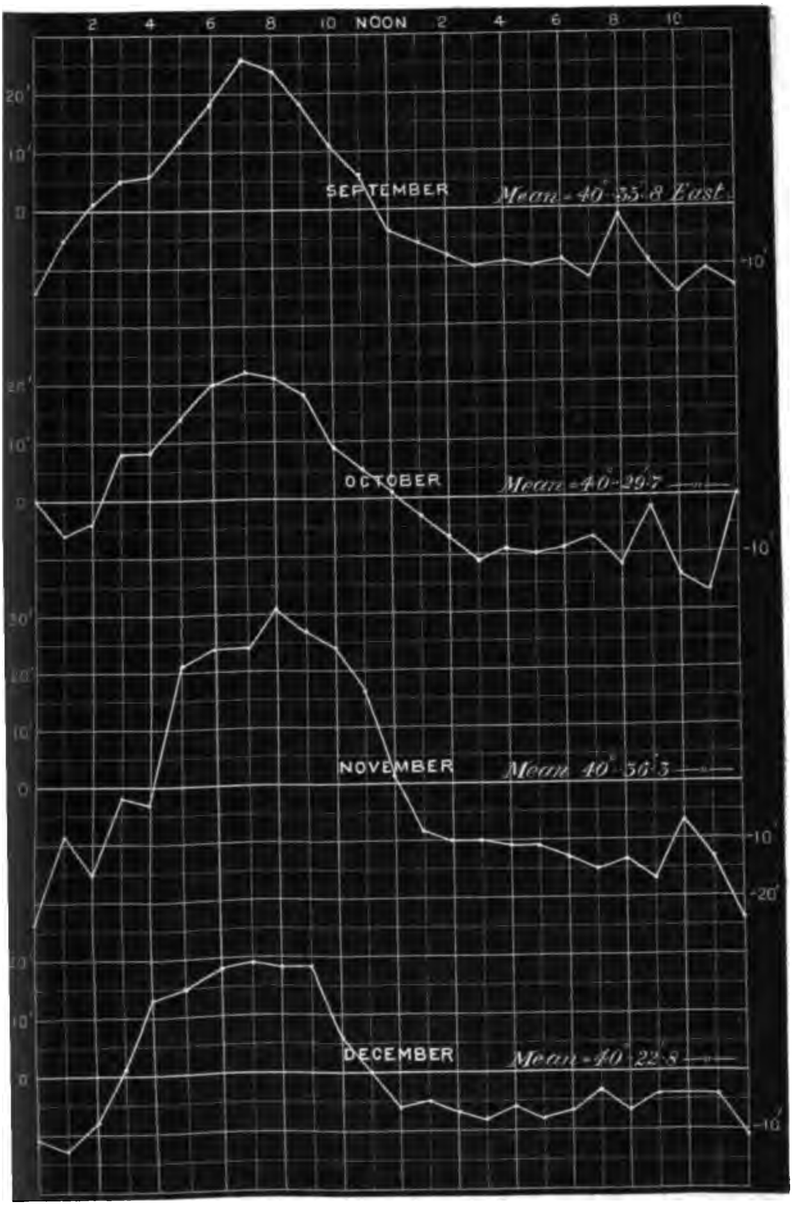
II. "On the Mean Diurnal Variation of Magnetic Declination, from Hourly Observations at Fort Rae." Communicated by Professor G. G. Stokes, Sec. R.S. By Captain H. P. Dawson, R.A. Received February 28, 1884.

The accompanying diagrams represent the mean diurnal variation of declination for each month from September, 1882, to August, 1883, at the Circumpolar station at Fort Rae, British North America.

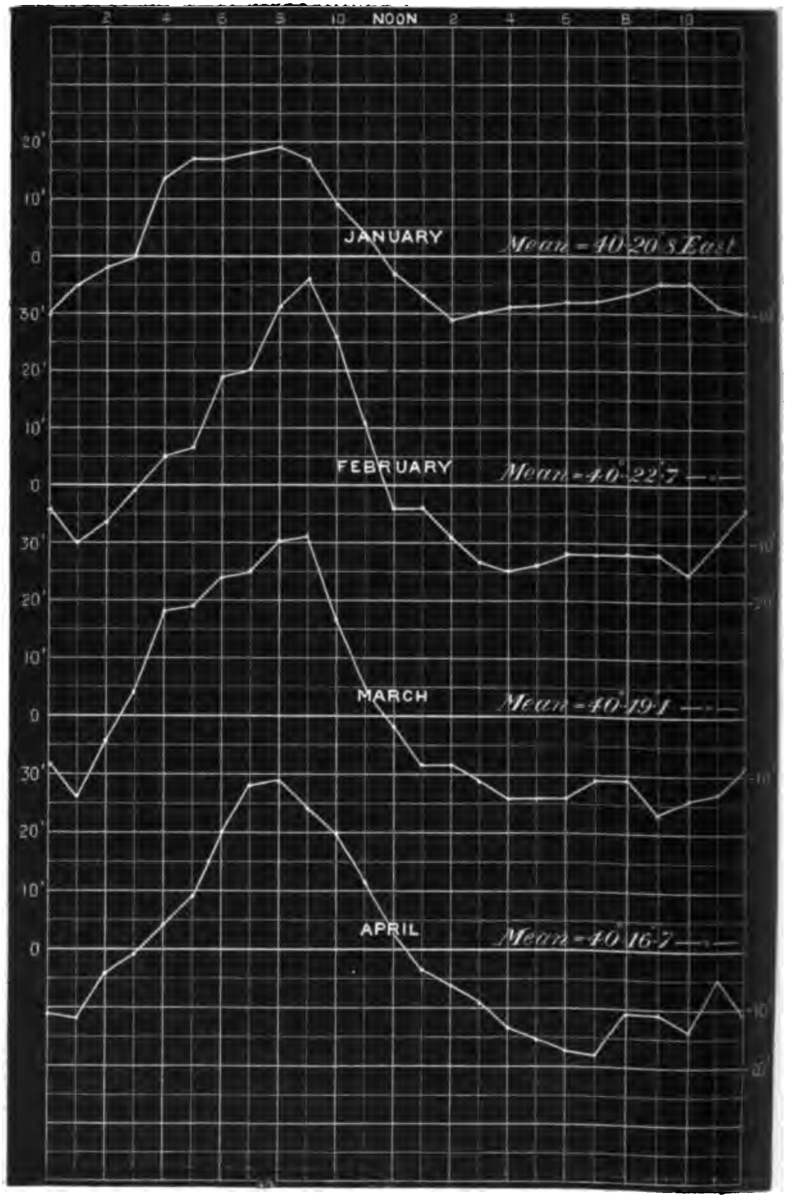
These curves are plotted from the hourly means for each month, and include the effect of disturbances which have greatly influenced them during the winter months, especially in November.

One effect of an unusual amount of disturbance is an increase of the mean declination; the easterly disturbance, or that tending to increase the declination, being always in excess.

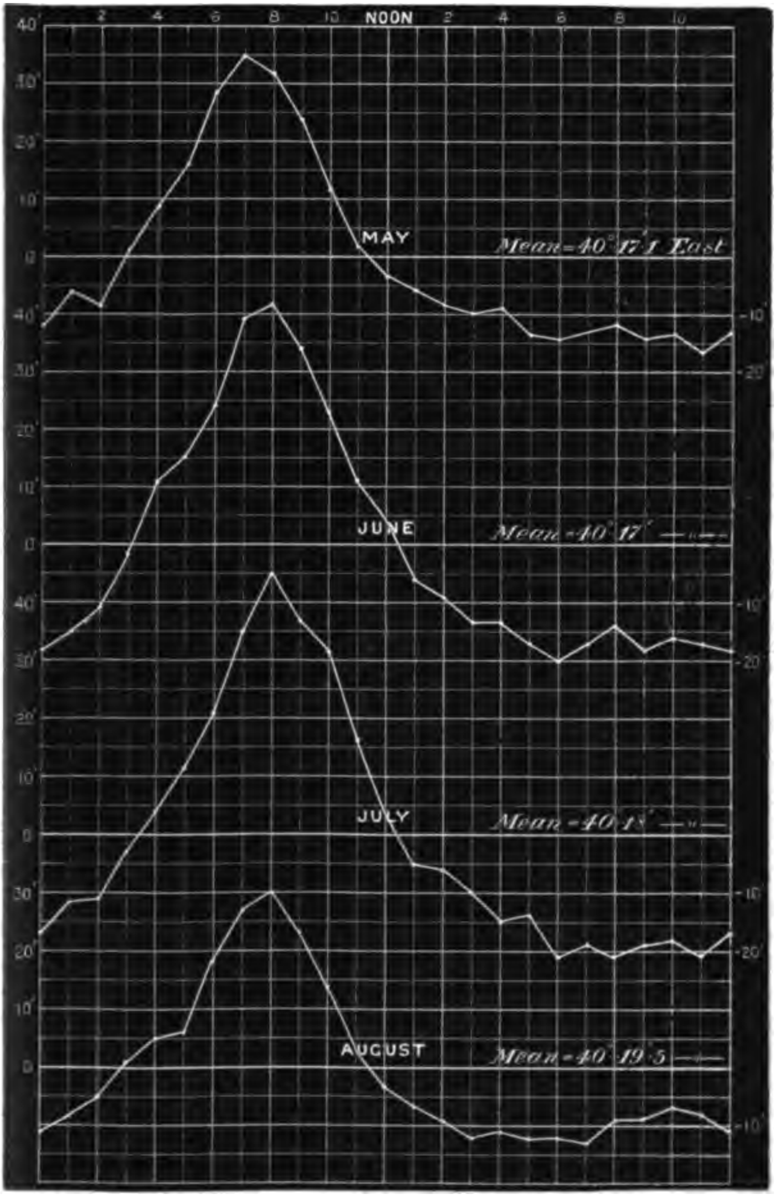
Mean Diurnal Variation of Magnetic Declination from Hourly Observations at Fort Rae.



Mean Diurnal Variation of Magnetic Declination from Hourly Observations at Fort Rae.



Mean Diurnal Variation of Magnetic Declination from Hourly Observations at Fort Rae.



III. "Notes on the Microscopic Structure of some Rocks from the Andes of Ecuador, collected by Edward Whymper, No. II. Antisana." By Professor T. G. BONNEY, D.Sc., F.R.S. Received February 29, 1884.

Antisana is a much loftier and grander mountain than Pichincha, for its summit rises to an elevation of about 19,000 feet above the sea,* and the upper part of the mountain (some 4,000 feet) is covered with snow and glaciers. The crevasses on the latter are described by Mr. Whymper as being of an enormous size, probably the largest he had ever seen, and on his first attempt to ascend the peak he was prevented from reaching the summit by chasms and cliffs of ice, among which his party, in consequence of the mists, had become entangled. A second attempt proved successful, but the snowy summit of Antisana is evidently not one likely to be reached by unpractised mountaineers.

The mountain is situated slightly to the south of the equator, to the east-south-east of the city of Quito, and nearly due east of the town of Machachi. "The extent of ground covered by Antisana," according to Mr. Whymper, "is, perhaps, as great as that covered by any of the Ecuadorian Andes, and more than is occupied by most of them. From north to south it extends over more than 20 miles of country, and not much, if at all, less from east to west. From most points of view at a distance, the mountain in form appears more like a ridge than a single summit. A close approach on the western side shows that this appearance is somewhat misleading, and that Antisana has two principal summits, the larger and higher being an immense snowy flat-topped boss, and the second (not less than 1,500 feet lower than the other) a sharp peak, which is probably at all times completely inaccessible."

The lowest point to which the glaciers extend on the western side is 15,294 feet; on the northern and southern they descend to about the same level; but Mr. Whymper is not able to say how far they come down on the eastern side. "In that direction the most notable feature of the mountain is a very extensive shoulder running out from near the summit, at a height of about 17,000 feet above the sea, in an east-north-east direction. It is singularly level and unbroken in outline, and is entirely obscured by a snow-covered glacier, suggesting by its form that there is an old flow of lava concealed beneath.

"There is no trace of a crater anywhere near the summit upon any side, but the snow covering the flat-topped boss, forming the higher

* Whymper, merc. bar., 19,335 feet; Reiss and Stübel, Δ, 18,885 feet. It is thus about third or fourth in order of elevation among the summits of Ecuador, being about as high as Cayambe, but lower than either Cotopaxi or Chimborazo.

point, may possibly fill and hide a crater. The diameter of the nearly level area which forms the summit is about equal to that of the lip of the crater of Cotopaxi. It is also certain that there is no open crater on any part of the western slopes of the mountain." Of the remainder Mr. Whymper says: "I speak with less confidence of the northern and southern sides, as I have not seen completely round them, and of the eastern side I can only speak of the parts not more than 1,000 feet below the summit. Upon my first and unsuccessful attempt to ascend the mountain we were stopped for a considerable time (by the difficulties encountered) at a height of somewhat more than 17,000 feet above the sea, and, whilst waiting, we noticed several puffs of strongly sulphurous vapour. We did not, however, observe either upon the summit, or when viewing it from a distance, anything of the nature of an eruption, or learn from the persons living in the vicinity of the mountain that any eruption had occurred to their knowledge."

Mr. Whymper's collection from Antisana consists, as in the case of Pichincha, of a series of ten specimens obtained in Quito, and fourteen collected by himself. Four of the latter shall be described first, as they come from the lower part of the mountain, from a spot called Antisanilla, which however is 12,340 feet above the sea. Here a *hacienda* abuts against a great lava stream which has descended from the mountain, and is the one most familiar to the natives of the district. Mr. Whymper remarks that it was the only large stream of lava which he observed on the western side of the mountain running on towards the west: "Its full extent I do not know, owing to mist. We coasted its southern side for 5 or 6 miles on the way to the Hacienda of Antisana (13,300 feet), and the small Hacienda of Antisanilla, an appendage of the large establishment, is built by the side of the lava stream, which was by various persons several times termed in my hearing the lava of Antisanilla. The surface of the stream was extremely rugged and well-nigh inaccessible."

From this lava Mr. Whymper collected specimens: the one selected for microscopic examination is a black sub-vitreous rock, containing small crystals of white felspar, whose diameter is commonly not more than 0·125 inch. The general aspect of the specimen shows it to be one of the darker varieties of andesite, a member of the group of rocks that have been variously named melaphyre, pitchstone-porphyrite, &c.

The crystals belonging to the earlier stages of consolidation which are included in the slide are rather small, no one of the felspars exceeding 0·1 inch, and only one or two approaching this size. They are plagioclastic, but as the majority have broken away in grinding the slide, one cannot venture to give a more definite name. The crystals of pyroxenic minerals are yet smaller; most of the latter occurring either in small scattered crystals about 0·006 inch long, of rather

elongated form or in small granules. Owing to the smallness and rather indefinite character of the pyroxenic constituents it is difficult to speak very positively about them; augite, however, is certainly present, and possibly hypersthene. Minute crystals and grains of an iron oxide, probably magnetite, as we might expect from the colour of the rock, are rather abundant. The ground-mass appears to consist of a clear glass, faintly tinged with brown, and densely crowded with microliths. These are lath-like crystallites of felspar, generally not exceeding 0·005 inch long, not seldom composed of two or three individuals, and belonites, of a very faint green tinge, not exceeding about 0·001 inch by 0·0002 inch, probably hornblende. The evidence as to the felspar is conflicting; probably both oligoclase and labradorite are present, but my observations tend to the conclusion that the latter is the more abundant species. The rock on the whole agrees best with augite-andesite. Its specific gravity, determined for me by Mr. J. J. H. Teall, is 2·656.

A second specimen from the same locality resembles the former in structure, but is of a dull india-red colour. It is so evidently the same rock, differently coloured by conversion of the black oxide of iron into the red oxide, that I have deemed it needless to examine its microscopic structure. The two other specimens are simply scoriaceous varieties of the latter rock.

It is difficult to fix the precise localities of most of the specimens obtained by Mr. Whymper from the collector at Quito, as the places mentioned on the labels are not known to the former, and in most cases, he thinks, are of no more general acceptation than the names attached by Alpine herdsmen to the crags and pinnacles in the vicinity of their châlets. All, he believes, are from the south-western or western side of the mountain and from localities whose height above the sea is not likely to exceed about 13,000 feet. Three are probably derived from some one subsidiary crater on the south-western flank of Antisana, named Guagra-ialina, though there is a slight variation in the spelling. The first of these is labelled *Corriente de lava de Guagrahialina volcan, Lado S.O.O. Antisana.* It is a dark grey rock, of scoriaceous aspect, with many small vesicles, usually less than 0·1 inch in diameter, and several specks of whitish felspar. It resembles some of the dark grey lavas of Auvergne, and, like them, is no doubt an augite-andesite. As the specimen presents no features of special interest, I have not examined it with the microscope. The next is simply labelled *Antisana, Guagrayalina volcan.* It is a compact dull grey rock, with a slight purplish tinge, containing occasional crystals of glassy felspar, sometimes rather more than 0·1 inch in length. These, on examination with the microscope, prove to be a plagioclastic felspar, but there is so much variation in the extinction angles that it is impossible to decide upon the species. They con-

tain, in variable amount, cavities with bubbles, brown glass enclosures, and other microliths, some being elongated prisms which may be apatite; but probably more than one mineral is present. There are also some small fairly well-defined crystals of augite, but I have not succeeded in identifying any hypersthene. A remnant of a glassy base appears to be present in the ground-mass, but it is so crowded with felspar microliths, and with granules of iron peroxide and of augite, as to be with difficulty distinguished. The felspar microliths are lath-shaped; they are a plagioclase; but, as in the case of the larger crystals, it is probable that more than one species is represented. The rock is an augite-andesite, and its general aspect reminds me of some of the porphyrites of the Cheviots (*e.g.*, a hypersthene-andesite from Coquet, near Windy Haugh). The third specimen, labelled *Guagra-ialina volcan, lado S.O. del Antisana*, is a rather duller coloured, less markedly porphyritic rock than the last, having some minute vesicles. The microscopic structure does not differ materially from that of the last described. Possibly a little hypersthene is present, but this is not conspicuous; thus the rock is an augite-andesite, and all these specimens may have come from different parts of the same flow or from a closely related series of flows.

From *Quebrada de Urcucuy* come two specimens of pitchstone. One, labelled *entre Tablarumi y Urcucuiloma*, is a dark greenish-grey rock, traversed by numerous cracks; its fracture is very irregular, and it exhibits the resinous lustre characteristic of pitchstone. A few minute scattered crystals or grains of a glassy felspar are visible, and there is a very faint indication of a fluidal structure. When examined microscopically, the rock exhibits as a base a clear and colourless glass. In this are scattered a large number of microlithic enclosures together with some scattered crystals of larger size. The general parallelism of the longer diameters of both of these, and the occasional filamentous streaks of an aggregated grey dust render the fluidal structure more conspicuous microscopically than macroscopically. The great majority of these microliths are little prisms or columns, usually about 0·001 inch long, and commonly about one-sixth of this in breadth. They are almost colourless, but appear to have a slightly green tinge. I think it probable that, like the belonites in the Arran pitchstones, to which they present some resemblance, they are hornblende. Besides these, we find opacite, with occasionally a fleck of brown mica or felspar crystals of small size. The "dusty" bands are found to resolve themselves, when viewed with a quarter-inch objective, into streams of microliths, like to, but perhaps slightly smaller in size than, those described above. Among the larger crystals are felspar: of this mineral orthoclase and a plagioclase are present. Some of the crystals are rather broken or rounded in outline, but others have well-defined external angles; the

latter are generally smaller and clearer, containing a few belonites, and but little else. The former are often "dirty," containing glass enclosures, cavities, and various microliths, as if belonging to an earlier stage of consolidation. Besides these are several crystals of brown mica and a few of hornblende, well defined, together with scattered grains of magnetite. The cracks are marked by a pale green staining, and there are no indications of a perlitic structure.

The other specimen of pitchstone labelled *entre Tablarumi y Chacana* is of nearly the same colour as the last described, but contains many rounded whitish spots, roughly about $\frac{1}{10}$ to $\frac{1}{8}$ inch diameter, which are seen on examination to be spherulites; a portion of the specimen is vesicular. The description given of the base of the last specimen will serve for this, except that there is little indication of a fluidal structure. There are a few scattered crystals of felspar, brown mica, and hornblende. The spherulites are rather peculiar, they have a rather irregular bluntly lobed outline, are nearly opaque, but exhibit a faintly fibrous structure, something like that of groups of blunt-pointed camel's hair brushes. So far as can be ascertained, they consist of a brown glass traversed by belonites of a paler mineral, and trichites of a darker one, but it is very difficult to determine their exact structure. They generally enclose a small crystal of hornblende or felspar, in one case both are present, but not centrically disposed. Without chemical analysis one cannot decide whether these two rocks are glassy forms of the rhyolites or of the dacites, but I should be disposed to class them with the latter.*

A third specimen from *Quebrada de Urcucuy*, labelled in addition *Entre Tablarumi y Urcucuy*, is a crumbling pale cream-coloured rock, which, on closer examination, gives indications of having been glassy and of a somewhat perlitic structure. This is confirmed by microscopic examination, though the nature of the rock has prevented the preparation of a good slide. It is evidently a decomposed perlitic pitchstone, and very probably when in a fresh condition was nearly related to the two others from this neighbourhood.†

From the south western side is a specimen labelled *Quebrada azufre grande, S.O.* Reiss and Stübel, as Mr. Whymper informs me, mention a "*Quebrada azufre grande*," giving a measurement *Parte inferior de la Loma al lado derecto de la Quebrada, &c.*, 4,040 mètres (13,255 feet). The name signifies "Great Sulphur" ravine. This

* Since the reading of this paper Mr. J. J. H. Teall, F.G.S., has kindly determined for me the specific gravity and silica percentage of the former of the two pitchstones. S.g. = 2·337; SiO_2 = 72·99, the loss on ignition being 1·15. These determinations fully confirm the microscopic analysis.

† A spherulitic pitchstone from Antisana is described by Vom Rath, "Verh. Nat. Ver. Preuss. Rheinl.," Folg. 4, Bd. 1; "Sitzungsb.," pp. 173, 174 (1874).

altitude is a little lower than the Hacienda of Antisana* (13,300 feet), and so, if the indication of direction be correctly given, cannot be so near as it to the summit.

The specimen is a rather compact cream-coloured rock, at first sight not unlike one of the South Tyrol dolomites, but slightly vesicular in places, and spotted here and there with pale yellow sulphur. It is evidently a volcanic rock from the vicinity of fumaroles which deposit sulphur, and presents the usual aspect of a trachyte which has been thus treated. From the appearance of the rock, and a certain resemblance to one of those described, from Quebrada de Urcucuy, I should think it probable it had once been a pitchstone.

A specimen is labelled *Del Nevado Pic O, principio del Arenal;* the "snowy peak" and the "sandy plain" of Antisana are localities unknown to Mr. Whymper. The rock is subvitreous, dark in colour with slightly redder streaks, and numerous scattered crystals of white felspar, commonly not more than about 0·1 inch long, but now and then twice or thrice as large. In the earliest stage of consolidation are (1) plagioclase felspar (probably in part at least labradorite), sometimes irregular in external form, often crowded with glass cavities, having fixed brownish bubbles, with microliths of augite (?), and with opacite; (2) augite; (3) very characteristic crystals of hypersthene; (4) granules of iron oxide and a few scales of iron glance. The rock has a glassy base, but this is crowded with lath-shaped felspar microliths (plagioclase), and in most parts is rendered almost opaque by dusty opacite and ferrite, the redder streaks being the more transparent parts, in which a glass, now clear, now brown, may be distinguished. The rock is a hypersthenniferous augite-andesite.

The locality of the next specimen, labelled *Cuspide del Achupallas Lado O. del Antisana*, is also unknown to Mr. Whymper. The rock has a deader lustre, and more scoriaceous aspect than the last described, and contains greater crystals of whitish felspar, their diameter being sometimes fully 0·3 inch. Under the microscope the larger of these are seen to contain glass enclosures and other microliths, and are probably labradorite; the smaller, which are more lath-shaped, agree better in their extinctions with oligoclase. There is a fair amount of well characterised brown mica and of hornblende, both brown and pale green varieties, with some granules of the latter or possibly of augite, and some grains of iron oxide. There is a clear glassy base,

* Reputed to be the highest farm in Ecuador. "It is situated on the western slopes of Antisana, in a cheerless situation, without a tree in sight, and is enveloped in fog the greater part of the year. The lower slopes of Antisana are of immense length and very devoid of character on this side. The upper 4,000 feet of the western side of Antisana is almost entirely covered by glacier. The nearest to the *hacienda* ends at an elevation of 15,295 feet." (E. W.)

but it is crowded with microliths of felspar, of a pyroxenic mineral, of brown mica, ferrite, &c. The rock is thus a mica-hornblende-andesite. The last specimen is labelled *Cuspide del chusa longo.* It is a dark grey vesicular rock, the proportion of solid to cavity being about two to one. The cavities commonly are not more than 0·2 inch in longest diameter, irregular in form, slightly drawn out in one direction, and coated with brown iron oxide. The rock is compact in structure, with a general resemblance to the matrix of the last described, but contains only very minute crystals of whitish felspar, rather irregular in form, and hardly more than 0·05 inch in diameter. It is no doubt an andesite, and is not unlike some of the scoriaceous varieties of that rock which are obtained from the Auvergne volcanoes. I have not thought it necessary to examine it with the microscope.

The remaining ten specimens brought back by Mr. Whymper are all representative of the highest part of Antisana. They were collected from the upper part of a moraine, by the side of which he encamped for the night, at an elevation of about 16,000 feet above the sea, or 3,300 below the actual summit. The materials of this moraine are derived from several rather small crags of rock which here and there crop out from the snowy slopes above. None of them were touched by Mr. Whymper during his ascent on the following day, for they are not numerous and are generally in inaccessible positions. He was careful to bring a specimen of every marked variety which caught his eye, so that the series is probably a fair representation of the rocks which constitute the Peak of Antisana.

Of these specimens (1) and (2) are vesicular rocks of a dull reddish colour, no doubt scoriaceous forms of a rock closely allied in composition to (4) and other dark varieties described below. (3) is a tuff, consisting of a fine yellowish paste, in which are numerous fragments up to the size of a small nut of a slightly vesicular, subvitreous, blackish rock, evidently closely allied to the next mentioned. (4) is a blackish subvitreous rock, containing glassy-looking felspar crystals up to about 0·2 inch diameter. A few minute vesicles are present. The microscopic description is given below. (5.) A very similar rock, a little lighter in colour, also described more fully below. (6.) Closely allied to the last, but paler, probably a little more decomposed. (7.) A dark compact rock, with some small crystals of felspar; very like the specimen from Antisanilla. (8.) A compact blackish rock, mottled with small spots of dull gray, in the inner part of which a small vesicle may be seen; a very few crystals of felspar, not exceeding 0·1 inch diameter, are visible; its microscopic structure is described below. (9.) A rather vitreous, slightly vesicular, rock, a fluidal structure being indicated by reddish and blackish layers, containing crystals of a whitish felspar, rarely exceeding 0·1 inch in diameter. The microscopic structure is described below. (10.) A large fragment

of dull reddish-gray not very vesicular scoria, probably lithologically in close alliance with (1) and (2).

The following is a description of the microscopic structure of No. (4). In the earlier stage of consolidation are (*a*) felspar crystals, probably in great part labradorite. The enclosures are frequently variable in nature, quantity, and arrangement. Sometimes their disposition is zonal and external, sometimes it is central. Among these enclosures are pale green belonites (? hornblende), colourless belonites, pieces of brown glass, often abundant, containing gas cavities and crystallites of magnetite, cavities containing bubbles, which occupy one-sixth or one-seventh of the whole space. The exteriors of the crystals are frequently broken-looking or corroded. (*b*) A pyroxenic constituent, of which some is certainly augite, but a part (the smaller) probably hypersthene. The former rather frequently contains enclosures; among them are magnetite and grains of a slightly irregular oval outline, sometimes nearly 0·003 inch diameter, occasionally associated with gas cavities (? felspar). (*c*) Grains of iron oxide, probably magnetite. The part of later consolidation is a pale brownish glass, speckled with opacite and crowded with acicular microliths, six or seven times as long as broad, which generally do not exceed 0·001 inch long. These are colourless and probably to a large extent felspar. No. (5) differs from the last rock but little in its microscopic structure; it has a rather clearer ground-mass and perhaps not quite so many granules of black iron oxide. The crystals of felspar are similar, but there is also a large number of well-formed lath-shaped crystals, measuring in longer diameter above 0·01 inch. Two varieties of augite, a greenish and a brownish, are present, together with a little of the greenish mineral which, as it has an orthorhombic extinction, I refer to hypersthene. Microscopic examination of (8) shows it to be not materially different from (4), except for the presence of the more decomposed spots, mentioned above. The glassy base is perhaps a shade more colourless. Both augite and hypersthene are present. No. (9.) In the first stage of consolidation we have rather numerous felspar crystals, with the usual variable enclosures—glass cavities with fixed bubbles, microliths, nearly all of which exhibit the characteristic twinning of plagioclase, though one or two show Carlsbad twinning and may be orthoclase. The former usually extinguish at moderately large angles, ranging from rather less than 10° to more than 20° with the twin-plane. In one, where the twinning is sharply defined, the extinctions are 21° and 30° respectively on either side of the twin-plane. It is therefore probable that these crystals are neither albite nor oligoclase. The pyroxenic constituent appears, as above, to be of more than one kind. The most abundant is a brownish, rather dichroic mineral, black bordered, and sometimes rather "dirty," owing to inclusions. In colour and general aspect it more resembles

hornblende, but the angles of cleavage (which, however, is in no case very well defined) and of extinction make it more probable that the mineral is augite. There are two or three crystals of a slightly greenish colour, which show the characteristic form and cleavage of augite, and one which in all respects better agrees with hypersthene. The ground-mass consists of fairly numerous, lath-shaped crystallites of a plagioclastic felspar, prisms of augite (?), often darkened with ferrite, granules of opacite and ferrite, and possibly in some cases flakes of mica. These are scattered in what may be a glassy base, but it is so densely crowded with extremely minute acicular crystallites (colourless, probably felspar), and with a minute dust (ferrite in the browner streaks, opacite in the darker) that, as the slide is rather thicker than usual, I cannot be quite sure. Although, to the unaided eye, and even when examined with low powers, this rock appears to differ considerably from (5) and (8), yet with high powers the resemblance becomes much closer, so that we may, I think, confidently refer it to the same group, and regard it as merely a more slaggy variety.

It follows, then, from the above examination that the rocks which form the actual peak of Antisana are augite-andesites, containing at any rate occasionally hypersthene, and to the same group belongs, though perhaps it is slightly more basic, the rock of the great lava stream which has descended to Antisanilla, while the pitchstones of Quebrada de Urcucuy must be representatives of a group with a higher percentage of silica, *i.e.*, rhyolites or dacites, probably the former.

IV. "The Variation of Stability with Draught of Water in Ships." By F. ELGAR, Professor of Naval Architecture in the University of Glasgow. Communicated by Professor Sir WILLIAM THOMSON, F.R.S. Received March 6, 1884.

(Abstract.)

Of all the properties possessed by a ship none is more vital to her safety and efficiency than that of stability. At the same time none is dependent for its existence and amount upon so many or such diverse and variable circumstances as it. The stability of a ship, both as regards moment and range, is affected not only by the position of her centre of gravity, which largely depends upon stowage, but also by draught of water. If the centre of gravity be kept fixed in position at various draughts of water, the stability will still vary very considerably with the draught, and often in a manner that contains elements of danger.

The usual practice in investigating a ship's stability is to calculate a curve of metacentres, and one or more curves of stability at certain

fixed draughts of water and with given positions of centre of gravity. The curve of metacentres gives the height at all draughts of water above which the centre of gravity cannot be raised without making the ship unstable when upright, and causing her to lie over more or less to one side. The ordinates of the curve of stability represent the lengths of the righting arms, which, multiplied by the weight of the ship, give the righting moments at all angles of inclination from the upright. The stability of numerous vessels, both of the Royal Navy and Mercantile Marine, have been investigated in this manner for certain draughts of water, and a great amount of information obtained respecting the variation of stability with inclination at such draughts, and the angle at which the stability vanishes in many classes of ships. The peculiar dangers attaching to low freeboard, especially when associated with a high centre of gravity, have been fully discussed and made known.

Curves of stability having been chiefly constructed for deep and moderate draughts, the character of the stability which is often to be found associated with very light draught, appears to have hitherto escaped attention. As a matter of fact, light draught is often as unfavourable to stability as low freeboard, and in some cases more so. The general opinions that have till recently prevailed upon the subject appear to have been based upon a vague impression that so long as a vessel has a high side out of water, and any metacentric height, she will have great righting moments at large angles of inclination and a large range of stability. It was shown at the "Daphne" inquiry, held by Sir E. J. Reed, in July last, that these opinions largely prevailed and were erroneous.

It fell to my lot to make some investigations respecting the stability possessed by the "Daphne" at the time of the disaster which happened to her, and to give evidence respecting the same. I afterwards pointed out, by way of explanation of my evidence, in a letter to the "Times" of the 1st September last, some of the considerations which obviously apply to light draught stability. The first, which so far as I am aware had never before been stated, is that any homogeneous floating body which is symmetrical about the three principal axes as the centre of gravity—such as a rectangular prism or an ellipsoid—will have the same moment of stability at equal angles of inclination, whether floating at a light draught with a small volume below water, or at a deep draught with a similar volume above water. For instance, if a homogeneous prism of symmetrical cross-section 5 feet high float at a draught of water of 1 foot, it will then have precisely the same moment of stability at equal angles of inclination, and consequently the same curve of stability throughout, as if it were loaded—without altering the position of the centre of gravity—till it had 4 feet draught of water, and 1 foot of freeboard. From this it

follows that, in such elementary forms of floating bodies, lightness of draught has the same effect upon stability as lowness of freeboard; and if a low freeboard is unfavourable to stability, so also, and precisely to the same extent, is a correspondingly light draught of water. This proposition can be made still more general, as it applies to homogeneous bodies of any form of cross-section which revolve about an horizontal axis fixed only in direction. From this may be deduced the results given by Atwood in his papers read before the Royal Society in 1796 and 1798 respecting the positions of equilibrium and other peculiarities connected with the stability of floating bodies.

In considering the stability of a ship at various draughts of water, and comparing it with that of the class of figures above described, modifications require to be made for the departure from symmetry of form, and for the extent to which the vertical position of the centre of gravity differs from what it would be if the external surface enclosed a homogeneous volume. This has been attempted in the present paper, and curves of stability, which I call cross curves, have been given for various geometrical forms of floating bodies, and also for a large passenger steamer of ordinary type, showing how the stability varies with draught of water at constant angles of inclination. In dealing with these cross curves of stability the curves of righting moments require to be constructed, and not merely curves of lengths of righting arm. The ordinary curve of stability is usually made for lengths of righting arm, because the displacement is constant, and the same curve therefore gives upon different scales, either lengths of righting arm or righting moments. In the cross curves of stability, however, such as are now being dealt with, draught, and therefore displacement, is one of the variable quantities, and curves of righting moments are of a very different character from curves of righting arm. The curves given in the figures are therefore, in all cases, curves of righting moments. Complete cross curves for a ship, from which ordinary curves of stability can immediately be obtained for any draught of water and position of centre of gravity, can be constructed in a few days with the aid of Amsler's mechanical integrator.

The main object of this paper is to show the necessity of regarding the stability of a ship from the point of view of variation of righting moment with draught of water, the angle of inclination being constant, instead of from that of variation of righting moment with angle of inclination, the draught being constant, as is usually done; or rather of considering the subject from both points of view instead of almost exclusively from the latter. It also shows that it is necessary to investigate more fully than has formerly been done, the moments and range of stability of ships and other structures that may be intended to float at very light draughts of water.

March 20, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Experimental Researches in Cerebral Physiology." By VICTOR HORSLEY, M.B., B.S., F.R.C.S., and EDWARD ALBERT SCHÄFER, F.R.S. Received March 6, 1884.

I. *On the Functions of the Marginal Convolution.*

(Preliminary communication.)

The present communication is intended to be the first of a series giving the results of an experimental investigation which we are at present engaged upon, into the physiology of the cerebral cortex and its connexion with other portions of the nervous system. We propose in this way briefly to publish any general results which appear to us to be well enough substantiated, as they are obtained; reserving most of the details of the experiments for a more complete memoir in which the various facts which may have been accumulated can be collated, and compared with the results obtained by other experimenters.

In the present research we have closely followed the methods employed by Ferrier. The animals used have been monkeys, most, if not all, some species of Macacque. In some the portion of the brain under investigation has been stimulated by the interrupted (induced) current, and the resulting movements recorded; in others (two in number) the cortex has been removed over the region in question, the removal being effected by the aid of the galvanic cautery and under antiseptic precautions, and the resulting pareses of voluntary movement observed. It was found disadvantageous to attempt both these observations upon the same individual, partly on account of the relative prolongation of the operation and the consequent danger of losing the animal from the resulting shock, partly because the carbolic spray which is used when it is intended to preserve the animal, appears temporarily to depress the functions of the portions of the cortex which are exposed to its influence, and either no reaction is obtained on stimulating them or a stimulus must be employed so

strong as to involve the risk of its spreading to neighbouring parts. The anæsthetic used has generally been ether, sometimes mixed with chloroform; in one case in which morphia had been employed the results of stimulation were much interfered with by the drug. The induction coil used is of the du Bois-Reymond pattern, with the Neef interrupter; and the Helmholtz side-wire is always introduced for the purpose of equalising the effects of the make and break shocks. The electrode wires are carefully guarded except at their points, which project slightly on one side, and the electrodes are so constructed as to pass between the falx and the mesial surface of the brain with as little disturbance as possible.

We have for the most part throughout all our experiments taken care only to employ an excitation just sufficient to call forth the activity of the part of the brain immediately under the electrodes. The best physiological test of the strength of the interrupted current consists in placing the electrodes on the tongue, and many of our results have been got with a stimulus which is only just perceptible when tested in that manner. Results so obtained are of the greatest value for the purpose of localising the function of a part, because under these circumstances there can be no question that the current does not spread beyond a very small area; and with such a minimal stimulus applied for but a short time it is not unfrequently found that but a single muscle, or at most two or three, generally in succession, are called into action. But for thus exactly localising centres for individual muscles a much larger number of experiments will be necessary than we have up to the present been able to make; we will, therefore, reserve for a future communication, in which we hope to deal more fully with this question, the few positive results of this kind that we have obtained, and here only consider the more general movements called forth by excitation of particular parts of the convolution.

We have in this way explored the mesial surface of the hemisphere, or rather the marginal convolution of that surface, for it soon appeared evident that on other parts of the mesial surface positive results were not to be expected from electrical excitation. We have ascertained that the excitation of definite localised portions of this convolution gives rise to the contraction of perfectly definite groups of muscles, or in some cases of single muscles, producing more or less co-ordinated movements of the trunk and limbs, in the same manner as has been shown by Ferrier and others to be the case with excitation of localised portions of the external surface of the hemisphere.

We can best explain the extent of the excitable portion of the marginal gyrus by reference to certain easily recognisable furrows on the external surface of the hemisphere. One of the most conspicuous of these is the furrow of Rolando, which, as in man, terminates superiorly near the margin of the hemisphere, a little in front of the posterior

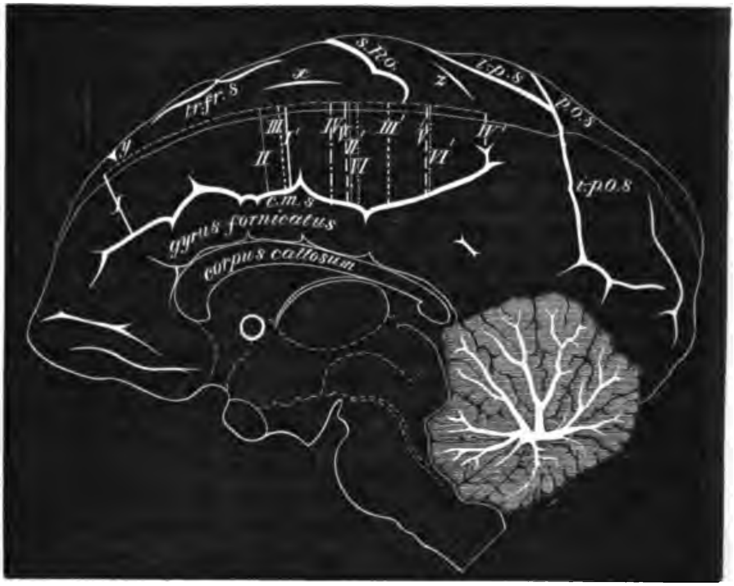
up-turned end of the calloso-marginal sulcus on the mesial surface. Behind the furrow of Rolando and opposite this end of the calloso-marginal furrow there is a small, but constant, obliquely-placed depression (fig. 1, *s*), which serves to mark the separation between the ascending parietal gyrus and the parietal lobule. In front of the furrow of Rolando, and separated from it by the upper end of the ascending frontal gyrus, is another small depression (*x*), also extremely constant in occurrence, although varying considerably in development, having a direction parallel to the margin of the hemisphere, from which it is a few millimetres removed. Still further forward, and at a much greater distance from the margin, is the well-marked transverse frontal furrow, the antero-posterior limb of which (*tr. fr*) is shown in the figure. And in front of this again is another small and apparently unimportant depression (fig. 1, *y*), but very constant in the Macacques, which has a transverse direction, *i.e.*, perpendicular to the hemisphere-margin, and which, in some instances, comes nearly up to the margin, conducting a considerable vein towards the longitudinal sinus. The excitable portion of the marginal convolution extends from about opposite this small transverse sulcus (F) backwards along the whole length of the convolution. In front of the level of the sulcus *y* no movements are, as a rule, obtained as the result of electrical excitation.

A remarkable relation was found on the whole to hold good between different parts of this convolution and the parts of the body thrown into movement by their excitation, to the effect, namely, that when the stimulus was applied anteriorly the resulting movements affected the upper limbs (and in one or two instances muscles of the head and neck); when applied near the middle of the excitable part of the convolution, the muscles chiefly or primarily affected were those of the trunk (erector spinæ, abdominal muscles, &c.), whilst, when applied posteriorly, muscles of the lower limb alone were called into action. Indeed, it appears probable that, if we regard only the results of minimal excitations, and especially if we take into account only those muscles which are primarily called into action, this rule will prove to obtain in a still more special manner, and that we may arrange the movements which are produced by stimulation of points which succeed one another from before back in the following order, viz.: 1. Movements of the forearm. 2. Movements of the humerus and scapula. 3. Movements, chiefly rotation and flexion, of the upper part of the trunk. 4. Movements of the lower part of the trunk and abdomen. 5. Movements of the pelvis. 6. Movements at the hip. 7. Movements at the knee. 8. Movements at the ankle-joint. 9. Movements of the toes.

The general results of our experiments will best be understood by a reference to the accompanying figure (fig. 1). Thus, in the part of

the marginal convolution marked I, I′, extending from just in front of the small vertical sulcus *y* to a point on a level with the anterior third of the small antero-posterior sulcus *x*, excitation is followed by either movements of the forearm (flexion or extension) or by adduction of the arm and retraction of the shoulder combined with outward rotation, or by any of these movements of shoulder and arm either combined or succeeding one another in definite order, according to the point in the area which is stimulated. Retraction of the shoulder (combined with flexion of the forearm) is alone produced by excitation of the posterior portion of this area, and when manifested as the result of excitation applied here, is apt to be associated with movements of the trunk, pelvis, or hip, which, as the overlapping of the contours of the areas shows, may also be called forth by excitation of this part.

Fig. 1.



In the next area, II, II′, we get movements of the trunk muscles as the result of excitation, the chief effect produced being a rotation of the body to the opposite side to that stimulated, combined with an arching of the spine, with the concavity directed towards the opposite side. In the anterior part of the area the chief effect is upon the dorsal region, but in the posterior part it is upon the lumbar region and pelvis.

This area is largely overlapped by the next one, III, III′, excitation within which is followed by movements of the hip, at some points the flexors only, at others the extensors only, at others both sets of muscles being called into contraction simultaneously. As will appear from the overlapping of the areas in the figure, these movements of the hip are apt to be associated with the rotatory and bending movements of the trunk above mentioned; but in the anterior part of the area it is generally the rotation of the trunk and pelvis which is first seen, and this is followed by hip movements, whereas in the centre of the area movements of the hip may be the first to appear, or with a very weak excitation may be the only ones visible.

The next area, IV, IV′, is very extensive. It considerably overlaps the areas II, II′, and III, III′, and extends to the posterior limit of the convolution. Its excitation calls up contractions of the thigh muscles, and especially of the hamstrings, which in some parts are the only muscles affected by weak stimulation—indeed, in some instances the contractions of the individual hamstring muscles were perfectly localised. But in most parts of the area, as the overlapping of the contours shows, these movements are associated with those of other muscles, viz., anteriorly with the trunk and hip muscles, and posteriorly with muscles which move the ankle and toes. These associated movements may be simultaneous, but are most commonly successive, as when by stimulation of one point there was produced, first a contraction of one of the abdominal muscles, then of one of the thigh muscles, and then of one of the muscles which move the ankle.

In like manner the area marked V, V′, may be looked upon as the specialised part from which the movements of the ankle are controlled, these being usually the first to appear on exciting the area, although very generally associated with or followed by movements of the hip and knee. And VI, VI′, may for a similar reason be looked upon as specially controlling certain movements of the toes, generally associated, however, with other movements of the lower limb. As before mentioned with regard to the other areas, the particular movements called forth differ according to the point in the area which is excited, but our experiments do not as yet enable us to make sufficiently positive assertions as to the localisation of these specialised points.

In the two animals from which the excitable portion of the marginal convolution has been removed, the resulting pareses of voluntary movement, so far as these can be determined, are precisely such as might be expected to occur from removal of those portions of the cortex by which the voluntary movements of the muscles which are called into action by stimulation of this convolution may be assumed to be governed. Since, however, as Ferrier has shown, certain of the muscles are also caused to contract by excitation of portions of the external surface, the paralysis of these would not be so complete as of

those which are solely connected with this convolution. Accordingly it is found that there is a considerable difference in the amount of paralysis for voluntary movement produced in the different muscles, and especially that some of them are found to undergo a considerable amount of recovery in the course of a relatively short time, while others remain permanently and completely paralysed. The paralysis (for volitional impulses) is most pronounced in the muscles of the toes and hind feet, and in the hamstrings and glutæi. The paresis is sufficiently obvious, but less marked in the arm-muscles than in those of the lower limb, while in the trunk-muscles it is extremely difficult to determine what movements are purely voluntary, what are associated movements, and what are purely reflex movements. We are unable, therefore, to say positively how far the influence of the will over these muscles has been abolished by the establishment of the lesion.*

It will, therefore, be more advantageous to defer the complete account of the condition of these animals until the opportunity is afforded by post mortem examination of verifying the extent of the lesion and of tracing the resulting secondary degenerations.

II. "Preliminary Note on the Apex of the Leaf in *Osmunda* and *Todea*." (From the Jodrell Laboratory, Royal Gardens, Kew.) By F. O. Bower, F.L.S. Communicated by W. T. Thiselton-Dyer, C.M.G., F.R.S. Received March 7, 1884.

It has long been accepted, in accordance with the investigations of Sadebeck, that there is at the apex of the young leaf of the fern a two-sided, wedge-shaped, apical cell, and that, after this cell has lost its identity by periclinal, and subsequently by anticlinal divisions, the growth of the leaf is continued at the margin by the persistent activity of a linear series of marginal cells. It is true that this is the mode of development of many fern-leaves, but, as my observations show, it does not apply for all cases, while those exceptional cases are particularly interesting as occupying an intermediate position in this, as also in other, respects between the true ferns, on the one hand, and the *Marathaceæ* and *Cycadeæ* on the other. It is among the *Osmun-*

* Since the above was written we have removed in two other animals the excitable portions of the external surface, in addition to the excitable portion of the marginal convolution. Complete hemiplegia has been the result; the paralysis affecting not only the muscles of the limbs but also those of the head and neck and of the trunk, whereas in animals in which only the excitable portions of the external surface (the motor regions of Ferrier) have been removed the paralysis is but partial, and confined chiefly to muscles of the limbs.—(Note added March 20, 1884.)

daceæ that these exceptional cases occur. In the young leaves of *Todea superba* and of *Osmunda cinnamomea* it was found that the apex is occupied by a well-marked, *three-sided*, conical, apical cell, from the three sides of which segments are cut off in regular succession, as at the apex of the stem of *Equisetum*. The apical cell is so placed that one side faces the ventral side of the leaf, while the remaining two sides are obliquely disposed with regard to the dorsal side of the leaf. No clearly marked marginal series of persistently active cells have been found giving rise to the pinnæ, as is stated to be the case for the typical ferns. Further, there appears to be no strict relation between the points of origin of the pinnæ and the segments cut off from the apical cell. The pinnæ arise in acropetal order.

In itself no great importance is to be attached to the difference between a three-sided and a two-sided apical cell. For example, it has been clearly shown in a paper by Treub, on the vegetative organs of *Selaginella Martensii*, that the two forms of apical cell are to be found on different shoots of the same species. But in the case of the leaf of the fern, the whole development, as described by Sadebeck and by Ruy, is so closely connected with the existence of a two-sided cell that a departure from that arrangement is to be regarded as of more importance than would otherwise be due to it, and it appears to me to supply an intermediate step towards the more complex leaf of the *Marathaceæ* and *Cycadeæ*.

Finally, it is believed that this is the first described case of a clearly marked, three-sided, apical cell occurring in the leaf of any plant. Holle asserts that there is a wedge-shaped apical cell at the apex of the leaf of *Angioptris*, and describes it as being "of irregular cross-section." My own observations on this point, which will shortly be described in detail, show that there is no single, functionally active, apical cell in the leaf of *Angiopteris evecta*.

III. "On the most Widened Lines in Sun-Spot Spectra. First and Second Series, from November 12, 1879, to October 15, 1881." By J. N. LOCKYER, F.R.S. Communicated to the Royal Society at the request of the Solar Physics Committee. Received February 22, 1884.

(Abstract.)

A preliminary report by Mr. Lockyer, written before the reduction of the observations given in the present paper was complete, was read to the Royal Society on December 15, 1881, and printed in the Proceedings (vol. 33, p. 154). In the present paper the author describes the plan of the observations and of their discussion, and presents some general conclusions.

When observations of spot spectra were commenced in 1869, the original idea was to observe the behaviour of every line widened or brightened in the spectra of each spot.

It was soon found, however, that in this climate it was exceptional to do this completely on any one day. Still, when it can be done, it is most important to secure such observations, and accordingly a complete method of reduction of such observations was suggested, laid before the Solar Physics Committee, and published by them in their Report.

Laboratory observations soon indicated the importance of having a series of strictly comparable observations. It became obvious therefore that the observations would require to be considerably restricted.

One reason why it was important to obtain such a series was that they might be compared with the complete records of bright lines seen in prominences given by Tacchini and others.

This consideration led to the suggestion that it would be advisable to take only the most widened lines, by which is meant the lines relatively the most thickened in the spots: accordingly the six most widened lines in each of the two regions, F to *b*, *b* to D, have been taken on every available opportunity. In this way a number of strictly comparable observations have been obtained.

Besides these observations, attempts have been made to photograph the spectra of sun-spots, and several photographs have been obtained. In all H and K were seen reversed over the spots, just as Young saw them at Sherman, while the blue calcium line was not reversed. The dispersion employed, however, up to the present has not been sufficient.

Previous researches had shown—

1. That with increased density in the spot-vapours we might expect an increase in the number of lines radiated, and therefore absorbed, either in the case of one vapour or of a mixture of vapours.

2. That an increased quantity of any one vapour in a mixture would increase the number of lines visible in the spectra of that substance.

3. That since absorption will vary with temperature, and as absorption is effected at different heights in the solar atmosphere, where the temperatures are different, depending upon the height on the average, the lines observed will vary according to the position of the absorbing stratum.

Information on all these points could be obtained by the method proposed, if the lines belonging to each substance were separately discussed afterwards. An individual discussion of each substance then formed part of the plan of the work.

Some General Conclusions.

1. In the photographs of spectra of sun-spots the H and K lines were always reversed, while the blue calcium line was not.

2. On February 26th, 1880, faint lines not marked in Ångström's atlas appeared among the most widened lines.

3. A change took place in the spectra of the sun-spots in May and June, 1881, the old lines faded away and new lines appeared.

3A. The lines of iron fade away from the spots as the maximum sun-spot period is approached.

4. In October, 1881, a similar change took place, only much more abrupt.

5. During the second hundred observations a much greater number of Fraunhoferic lines have been seen wider than in the first, but with less frequency.

6. The most widened lines have shifted towards the less refrangible part of the spectrum.

7. In some spots certain lines have indicated change of refrangibility, while other lines in the same region have not done so.

7A. On several occasions certain lines of iron have been seen in motion, while other lines in the same field of view have been at rest.

8. There are immense inversions in the lines seen widened in the spots from spot to spot, and from day to day.

9. There is a great inversion between the iron lines seen among the most widened lines.

10. There are yet greater variations between the lines brightened in prominences and widened in spots.

11. On comparing Young's chromospheric lines with the most widened lines, it is found that the number of common lines increases till the end of the third period, and then it falls.

11A. There is a great difference between the lines seen in prominences by Young at the maximum and those seen by Tacchini at the minimum sun-spot periods.

11B. In the region between F and *b* there are no iron lines common to the most widened lines and Tacchini's observations of prominences.

12. Even with a six-fold complexity I was unable to classify the most widened iron lines.

13. The spectrum of iron in the sun is more like that given by the spark than that given by the arc.

14. Of three iron lines at 4918·0, 4919·8, and 4923·1 the two former have been seen in spots among the most widened lines, while Tacchini and others have seen the latter without the former in prominences.

15. The prominence line 4923·1 is sometimes seen under certain conditions brighter than the longest iron line, in the region at 4956·7.

16. The lines of iron, manganese, zinc, titanium, nickel, and copper most frequently seen in spots are different from those most frequently

seen in prominences, whilst in cobalt, chromium, and calcium they are the same.

17. The lines of iron, cobalt, chromium, manganese, titanium, and nickel seen in the spectra of spots and prominences are usually coincident with lines in the spectra of other metals with the dispersion employed, whilst the lines of tungsten, copper, and zinc are not.

18. All the lines of titanium seen among the most widened lines have either been greatly developed in passing from the arc to the spark, or else have been seen only in the spark.

19. The lines of titanium, zinc, and nickel, seen widened in the fourth period, are not the same as those seen in the first.

20. Several of the new lines seen among the most widened lines occupy positions near those occupied by titanium lines.

21. No lines of cobalt, manganese, chromium, copper, or tungsten, were seen in the fourth period.

22. A strong barium line has been seen once among the most widened lines; it is a line greatly intensified in passing from the arc to the spark.

23. A hundred and one lines have been seen among the most widened lines which have no corresponding lines (so far as is known) in the spectra of the elements. One of these lines has been seen frequently in prominences by Young.

24. So far as the observations have gone, there has been no difference caused by the nearness of the spot to the limb.

March 27, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Notes on the Varieties and Morphology of the Human Lachrymal Bone and its accessory Ossicles." By A. MACALISTER, F.R.S., Professor of Anatomy in the University of Cambridge. Received March 14, 1884.

(Abstract.)

The author has examined 1000 lachrymal bones and the soft parts of over 300 orbits, and has deduced therefrom notes on the following points:—

I. Cases of absence of the lachrymal bone.

II. Rudimentary conditions:—

- α. Development of an orbital plate only.
- β. Division into two detached ossicles.
- γ. Development of a crest only.
- δ. Fenestration.
- ε. Presence of marginal fontanelles.

III. Varieties in outline, saturation, and frequency of ankylosis.

IV. Varieties of crista:—

- A. Simple lamellar forms with no crest.
- B. Simple carinate forms.
- C. Carino-acuminate forms.
- D. Carino-hamate.

V. Measurements of the bone in adult male and female, and the deduction of lachrymal index $\frac{b \times 100}{l}$.

VI. The vascular relations of the bone, which are shown to be almost exclusively with the infra-orbital artery, not with the angular.

VII. The disposition of the periosteum and soft parts, and the relation thereto of the tendo oculi and muscles, as well as the relations of the bone to the neighbouring cavities in the ethmoid.

VIII. The accessory ossicles developed in its vicinity, which are six. One, a detached portion of the lachrymal itself, with a second ossific centre (ossiculum hamuli); one, a detached portion of the os planum behind it (ossiculum ethmo-lachrymale superius); two, which are detached nodules connected with the maxilla (ossiculum ethmo-lachrymale inferius and ossiculum canalis naso-lachrymale); one, which belongs to the system of the jugal bone (ossiculum infra-orbitale); and one, a detachment of the nasal process of the maxilla (ossiculum maxillo-frontale). To this last-named the sutura notha is always related.

IX. The history of the development of the human lachrymal from its first appearance in the eighth week, and the changes in position and slope shown by it.

X. The racial varieties shown by this bone, and the system of inter-lachrymal indexes whereby the degree of downward divergence and orbital splay of the bone can be measured.

XI. Comparative anatomy and morphology of the bone. Those points have been briefly referred to which help to interpret the human conditions.

II. "On the Electro-Chemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells." By Lord RAYLEIGH, D.C.L., F.R.S. Received March 17, 1884.

(Preliminary Notice.)

The investigations upon this subject which have been carried on by Mrs. Sidgwick and myself during the last year and a half, though not yet quite finished, are so far advanced that no doubt remains as to the general character of the results; and as these results have application in the daily work of practical electricians, it is thought desirable to communicate them without further delay.

The currents are measured by balancing the attraction and repulsion of coaxal coils against known weights, as described before the British Association in 1882, a method which has fully answered the favourable expectations then expressed. To what was said on that occasion it will be sufficient for the present to add that the readings are taken by *reversal* of the current in the fixed coils, and the difference of weights thus found (about 1 gram) represents the double force of attraction, free from errors depending upon the connections of the suspended coil, and other sources of disturbance.

The difficulties which have been experienced, and which have been the cause of so much delay, have related entirely to the behaviour of the silver voltameters, of which never less than two, and sometimes

as many as five, have been included in the circuit of the measured current. In order to render the deposit more compact, and thus to diminish the danger of loss in the subsequent manipulations, *acetate* of silver was added in the earlier experiments to the standard solution of nitrate. Experience, however, has shown that the principal risk is not in the loss of metal, but in the obstinate retention of salt within the fine pores of the deposit, leading to an over-estimate of the amount. When the texture is very compact this danger increases, and deposits from a solution containing acetate are often decidedly too heavy, even after the most careful and protracted washings. On heating to low redness a portion, at any rate, of the retained salt is decomposed, NO_3 is driven off, and a loss of weight ensues. With pure nitrate, to which we finally recurred, the risk is much less.

The actual weights of deposited silver were usually from 2 to 3 grms., and, so far as the mere weighings are concerned, should have been correct to $\frac{1}{10000}$. Discrepancies three or four times as great as this are, however, actually met with, whether due to retention of salt or to loss of metal it is difficult to say. The final number, expressing in C.G.S. measure the electro-chemical equivalent of silver, is a little lower than that ($1{\cdot}119 \times 10^{-2}$) given on a previous occasion ("Cambridge Proceedings" for November 26, 1883). It approximates closely to $1{\cdot}118 \times 10^{-2}$, and is thus in precise agreement with the number announced within the last few weeks by Kohlrausch, viz., $1{\cdot}1183 \times 10^{-2}$. Its substantial correctness can therefore hardly be doubted, more especially as it does not differ very much from the number (1·124) obtained by Mascart. In terms of practical units, we may say that the ampère current deposits per hour 4·025 grms. of silver.

When we are provided with means for the absolute measurement of currents, the determination of electromotive force is a very simple matter if we assume a knowledge of absolute resistance. A galvanic cell is balanced against the known difference of potentials generated by a known current in traversing a known resistance. The difficulty relates entirely to the preparation and definition of the standard cells. A considerable number of Clark cells have been set up and tested at intervals during the last six months, and their behaviour has been satisfactory, the extreme range (after the first ten days) not much exceeding $\frac{1}{1000}$. A modified form of cell in which the solid zinc is replaced by an amalgam, is at present under trial.

In Mr. Latimer Clark's own determination the B.A. unit is assumed to be correct, and the E.M.F. of the cell at 15° C. was found to be 1·457 volt. *On the same assumption*, we obtain the not greatly differing value 1·453 volt. If we take the true value of the B.A. unit as ·9867 ohm, 1·453 will be replaced by 1·434.

Experiments are also in progress to determine in absolute measure

the rotation of the plane of polarisation of light in bisulphide of carbon under the action of magnetic force. Of the results obtained by Gordon and Becquerel, differing by about 9 per cent., our preliminary measurements tend rather to confirm the former.

III. "On the Natural and Artificial Fertilisation of Herring Ova." By J. Cossar Ewart, M.D., Regius Professor of Natural History in the University of Edinburgh. Communicated by The President. Received March 19, 1884.

It is well known that for centuries herring have been in the habit of congregating on inshore banks around the coast of Britain in order to deposit their spawn.

In 1862 the Royal Commission (of which Professor Huxley was a member) appointed to report on the trawling for herrings on the coast of Scotland, arrived at the conclusion that herring visit our shores for this purpose twice a year, some shoals arriving during the autumn, while others make their appearance during the winter. The herring which spawn during the autumn (and which at another time I shall endeavour to show differ from the winter herring) chiefly frequent banks on the east coast, while the herring which spawn during winter are most abundant on the west coast.

Of the west coast spawning-grounds, the Ballantrae Bank, which lies off the coast of Ayrshire, is one of the most important and is certainly the most famous. To this bank herring are known to have resorted for at least 200 years, always bringing in their train numerous codfish, whiting, and sometimes shoals of dogfish, porpoises, and dolphins, and while on the bank they have afforded an abundant harvest to the fishermen of the surrounding districts, and to the flocks of gannets and gulls which people Ailsa Craig.

The herring fishery being one of the most important industries in Scotland (the autumn fishery engaging nearly half-a-million people, and being worth in good years about 2,500,000*l.* sterling), there has been since 1809 a Board specially charged with guarding its interests. This Board (formerly known as the Board of Fisheries, but since 1882 as the Fishery Board for Scotland) in 1862–63 endeavoured, under the direction of Professor Allman (then a member of the Board), to gain some information as to the habits of the herring, and more especially as to the nature of the spawn and the spawning grounds. Since 1863 little has been done in this country by way of continuing these experiments until last autumn, when the new Fishery Board, recognising the importance of the investigations so ably initiated by Professor Allman, appointed a Committee of its

members to continue the observations, and extend them so as to embrace as far as possible the consideration of all the food fishes.

But while little has been done in this country to increase by means of continuous observations our information as to the habits and life history of the herring, important results have been obtained by the German Fish Commissioners, and from observations made for the Norwegian and Swedish Governments and the United States Commissioner of Fish and Fisheries. Nearly all the work hitherto done is summarised in the valuable "Reports of the United States Commissioner of Fish and Fisheries," more especially in Parts III and VI, where Widegren, Lyungman, and Sar's researches are referred to, and a full account of D. H. A. Meyer and Dr. C. Kupffer's work will be found in the "Jahresbericht d. Commission z. wissenschaftlichen Untersuchung d. deutschen Meere in Kiel f. d. Jahre 1874, 1875, 1876," and the "Vierter Bericht d. Commission f. d. Jahre 1877 bis 1881."

During the autumn two members of the Fishery Board Committee (Sir James R. Gibson Maitland and myself) having had H.M.S. "Jackal" (Lieutenant Prickett, R.N., commander) placed at their disposal by the Admiralty, were enabled to examine the more important spawning-beds in the Moray Firth, and to make experiments with the view of determining the best mode of artificially fertilising and hatching herring ova. A preliminary report of the work done has already been presented to the Board ("Nature," 29th November, 1883), and a complete report will be published at an early date.

The Fishery Board, keenly appreciating the importance of the scientific work it had instituted last autumn, on learning that the winter herring fishery had begun off the Ayrshire coast, requested me, on the 3rd March, to join the fishery cruiser, H.M.S. "Jackal," at Girvan, with the view of inspecting the famous Ballantrae spawning grounds.

On reaching the bank I found that it was chiefly occupied by seine-net fishermen, and that, although the bank is about 3 miles in length and from 1 to 2 miles in breadth, the principal spawning and fishing ground were confined to a comparatively limited portion of that area. There being, in addition to seine-net, both trammel- and drift-net fishermen, and an abundant supply of mature herring, it was evident that Ballantrae was not only unique as a fishing ground, but that it afforded facilities for prosecuting the inquiries which the Fishery Board had instituted, such as were not likely to be found on any other part of the British coast. Accordingly arrangements were at once made (1) for taking temperatures and examining the nature of the bottom, and (2) for securing and keeping alive a number of mature herring to enable us to study the process of spawning, and to make experiments in artificial cultivation. At the outset it is only

right that I should say that Lieutenant Prickett took an equal share in all the work done, and that the success which followed resulted chiefly from the enthusiastic manner in which the officers and men entered into the spirit of the inquiry.

I. *The Spawning Ground.*

Professor Huxley, in his address at Norwich, says, "The first definite and conclusive evidence as to the manner in which herring spawn is attached and becomes developed, that I know of, was obtained by Professor Allman and Dr. McBain in 1862 in the Firth of Forth. By dredging in localities in which spent herring were observed on the 1st of March, Professor Allman brought up spawn in abundance at a depth of 14 to 21 fathoms. It was deposited on the surface of the stones, shingle, and gravel, and on old shells and coarse shell-sand, and even on the shells of small living crabs and other crustacea adhering tenaciously to whatever it had fallen on."* This spawn was dredged by the fishery cruiser "Princess Royal," Captain Macdonald, commander.

There is no record, as far as I know, of herring spawn having been dredged from 1862 until 1872. In a valuable paper on the Norwegian Herring Fisheries by A. J. Boeck and A. Feddersen (1872),† it is stated that Boeck "raised with the dredge large lumps of roe and gravel intermixed." The only other reference to the dredging of herring spawn I am acquainted with is in the "Report of the German Commission,‡ where it is mentioned that the eggs of the inshore herring were found in the Schlei at a depth of 3 feet, attached to a fresh water pond weed (*Potamogeton*). But although no record has been kept, herring ova must often have been taken up by our fishermen, for during the last three months I have obtained numerous small clusters of herring ova attached to sea-firs (chiefly *Hydrallmannia falcata*) at all stages of development, which had been brought up by the east coast long-line fishermen. Taking advantage of this knowledge, I prepared a number of small grapnels by tying three large cod-hooks together and fixed them by strong cords about 6 feet in length to a heavy 4-feet iron bar. With this rude instrument, an oyster-dredge, and an ordinary naturalist's dredge, we drifted or steamed slowly across the bank in various directions, and on several occasions succeeded in bringing up fine specimens of herring ova attached to stones, sea-weed, and sea-firs, and portions of trammel-nets.

* "Nature," April, 1881, and "Report of the Royal Commission on the Operation of the Acts relating to Trawling for Herrings on the Coasts of Scotland," Edinburgh, 1863.

† See translation in "United States Report of Commissioner of Fish and Fisheries," Part III.

‡ "Jahresbericht der Commission in Kiel für 1874–75."

The cod-hook grapnels brought up the trammel-nets (in which were a few recently caught herring and a large number of fish, chiefly herring, in an advanced stage of decomposition), and also most of the sea-weed; the oyster-dredge brought up all the stones coated with spawn. These stones varied from 6 inches to 1½ inches in length, and from 4 inches to 1 inch in breadth, but in all cases the eggs were attached to a comparatively smooth surface (which probably was uppermost when the eggs were deposited), and they were arranged either in low cones or in comparatively thin layers, one or two eggs deep.

The eggs on the laminaria and other sea-weeds were either lying separately or in small isolated groups, or embedded amongst tho roots, while those on the sea-firs were always attached in small clusters about half an inch in diameter around the stems.

On the trammel-nets the eggs were scattered in an irregular manner over several square feet. They were always most abundant near the fish from which they had escaped, and tailed away sometimes to the right and sometimes to the left, the position having undoubtedly been determined by the direction of the currents at the moment they escaped.

On examining the spawn found on the stones and sea-weed, embryos at various stages of development were at once visible, some of them apparently only three days old, while others had distinct eyes, and from their violent movements and their size seemed almost ready for hatching. Some of the egg-coated stones were preserved in spirit, others were deposited in the Rothesay Aquarium or retained on board the "Jackal," while others were conveyed to the University of Edinburgh. I have not heard what has happened to those left at Rothesay, but those left on the "Jackal" under the care of Lieutenant Prickett, hatched on the 13th, five days after they were dredged; and those carried off to Edinburgh (notwithstanding that their small supply of water was neither changed nor aërated for nearly twenty hours), hatched on the 15th March, eight days after their removal from the Ballantrae Bank, and to-day (17th March), the fry are three-eighths of an inch in length, extremely active, and swimming freely about in the water.

All the eggs on the trammel-nets and on the floats in connexion with the nets were dead; they looked as if they had never been fertilised, while some of those scattered over the smooth surface of the stones attached to the lower margin of the nets contained living embryos.

In addition to the stones, sea-weeds, and trammel-nets, the dredges brought up amongst other things numerous shells of Pecten, Buccinum, and Cardium, small specimens of Chiton, and the eggs of several nudibranchs attached to stones; several Echinoderms—

Asterias, Astropecten, Ophiura, Echinus, Antedon, and others—a few Crustacea—Pagurus, Nephrops, Palæmon, Cancer, and other crabs; a few sertularians and actinozoa, chiefly the common Alcyonium. Compared with the spawning-beds examined during the autumn in the Moray Firth, the Ballantrae Bank is peculiar in having as far as our experience went a very limited coating of sea-weed and sertularians. The fishermen at Ballantrae reported that owing to recent storms the sea-weed is less abundant now than it was some years ago.

The absence of sea-weed cannot be accounted for by beam-trawling, for so far as I could learn trawlers are careful to avoid the bank lest they destroy their nets. Perhaps the spawning-bed might be improved if it were possible to increase the amount of sea-weed and sertularians.

By taking soundings over the bank in various directions it was ascertained that it consisted of stones, shells, gravel, and coarse sand, and that the depths varied from 7 to 12 fathoms. Between the bank and the shore—a distance of about a mile—the bottom consists chiefly of sand and broken shells, and the depth, at some points 8 fathoms, diminished sometimes rapidly, sometimes slowly as the shore was approached. The outer edge of the bank shelved at most points rapidly until a depth of 17 fathoms was reached, and at this depth the bottom consisted of fine soft mud.

While, on the east coast spawning grounds examined during the autumn, the surface temperature in most cases varied from 53° F. to 55° F., and the bottom temperature from 52° F. to 54° F. even at a depth of 40 fathoms, the temperature at the Ballantrae Bank, during the early part of March, varied from 42°·8 F. to 43°·8 F. at the surface, and from 43°·5 to 42°·8 F. at the bottom.

The corresponding surface temperature, however, on the east coast during the week ending March 8th was from 2° F. to 3° F. lower than at Ballantrae, and even the water in Rothesay Aquarium was lower by 1° F. than the water at Ballantrae. As in autumn the temperature is believed to have a great influence in determining the movements of the herring, so it may influence the selection of spawning grounds during the winter.

During the autumn we found in the neighbourhood of all the spawning-beds in the Moray Firth a remarkably rich surface fauna, in fact on fine days the surface literally swarmed with life. In addition to the millions of medusoids and ctenophora there was an endless number of crustaceans (some of them at a very early stage of development), larval echinoderms and molluscs, ascidian tadpoles, and other minute pelagic forms. On the other hand, on the Ballantrae Bank, even when the surface was perfectly smooth, we only succeeded in taking a few entomostraca with the surface-net. There

is, therefore, as far as has been ascertained, little or no surface-food for the millions of herring-fry which will undoubtedly be soon hatched on the Ballantrae Bank. It may be possible to secure specimens of the larval herring, and thus ascertain what they are feeding on.

II. *The Natural Spawning Process.*

The presence of a large school of porpoises and dolphins, and the excitement and rocket-like movements of the gannets, taken together with the calmness of the sea on the afternoon of Friday, the 7th March, led us to expect that there would be a good night's fishing. We therefore arranged to be on the fishing-grounds early on the Saturday morning, well provided with large tanks for receiving live herring, and with hatching and carrying-boxes for their spawn. Our expectations having been realised, we had at our disposal on Saturday morning thousands of mature living herring. Selecting about fifty of the largest and ripest, they were placed in tanks on board the "Jackal," and conveyed as rapidly as possible to the Rothesay Aquarium. On the way there was time to make a number of experiments, and to cover several plates of glass with artificially fertilised ova, but before referring to these experiments, and the method adopted for artificially impregnating the eggs, I shall describe how the eggs are deposited and fertilised by the herring themselves. This, so far as I am aware, has not been previously observed. In the best account we at present possess of the structure and habits of the herring, Professor Huxley says, "When spawning takes place naturally the eggs fall to the bottom and attach themselves." "But at this time the assembled fish dart wildly about, and the water becomes cloudy with the shed fluid of the milt. The eggs thus become fecundated as they fall, and the development of the young within the ova sticking to the bottom commences at once."* Mr. Mitchell, in his book on "The Herring," referring to the once famous spawning-bed off Dunbar, states that, "About the 30th of August the shoals began to deposit their spawn a short distance from the harbour, and on the 3rd of September the fishermen found that a very large body of herrings remained fixed to the ground in the process of spawning, the ground being of a rocky or stony nature." It is not added how the fishermen knew the herring remained fixed to the ground. It is not likely that a diving-dress was used, but even if it had been, nothing could have been made out, as it is extremely difficult even in a good light to observe what takes place in an aquarium. While many fishermen believe that herring spawn on hard ground, some believe that they also spawn on a clayey bottom, and while some think that they spawn near the bottom, others affirm

* "Nature," April, 1881. See also "Jahresbericht der Commission in Kiel."

that they spawn near the surface, this latter being the favourite theory of Dutch fishermen.

In the preliminary report of last autumn's work already referred to, we give it as our opinion that "The spawn once ripe they congregate at the bottom, the females depositing their roe on the rocks and sea-weeds, to which it at once firmly adheres, and the males fertilising it with their milt."

But it will be observed we did not venture an opinion as to how the spawn was deposited, or how it was fertilised.

Having secured at Ballantrae a number of live herring, we selected the largest and ripest males and females, and placed them in a large wooden tank, into which a number of stones and a quantity of sea-weed had been previously introduced. After the fish had been about two hours in this tank we examined the stones and sea-weed. Although a few eggs were attached to both stones and sea-weed, it was quite evident that they had not been deposited in the same way as those found on the stones dredged on the previous day, but the presence of the eggs indicated that we had secured ripe females. We were not surprised that only a few isolated eggs were found on the stones, because the fish had been disturbed every few minutes by the water poured into the tank in order to keep them sufficiently cool and provided with an abundant supply of oxygen. Having arranged a tent-like covering over the tank, so as to inclose the fish in a dark chamber, we tried the effect of throwing instantaneous flashes of light on the surface of the water, but as far as could be observed this produced no impression whatever, they neither sought the light nor avoided it, neither did they seem in any way startled, however suddenly it was directed across their path. This led us to believe that their natural movements would not be seriously interfered with when introduced into the tanks at the Rothesay Aquarium.

On reaching Rothesay the hatching boxes and live herring were at once transferred from the "Jackal" to the tanks, a tank into which comparatively little light entered being selected for the ripest and most vigorous herring. In about half an hour after they were introduced I noticed a large full herring moving slowly about the bottom of one of the tanks, and thinking it had suffered during the journey, I introduced a landing-net, in order to remove it, when, much to my surprise, it darted to the opposite end of the tank. Having, however, without much difficulty, secured the herring, and having ascertained it was a perfectly ripe female, it was set at once free. In a few minutes I noticed her moving slowly quite close to the bottom of the tank, with four other fish making circles around her at some distance from the bottom. Appearing satisfied with some stones which she had been examining, she halted over them, and remained stationary for a few minutes about half an inch from their surface, the tail

being in a straight line with the trunk and the pectoral fins near or resting on the bottom. While in this position, a thin beaded ribbon was seen to escape from the genital aperture and fall in graceful curves, so as to form a slightly conical mass, almost identical with a cluster of ova on one of the stones dredged at Ballantrae. As the little heap of eggs increased, some falling to the left side one moment, while others fell to the right the next, according to the currents in the water, the males continued circling round the spawning female at various distances, while the other females in the tank remained apart. The males kept from 8 to 10 inches above the bottom of the tank and formed circles ranging from 18 inches to 30 inches in diameter. Some of the males were swimming from right to left, others from left to right, and although there was no darting about, no struggling among themselves (there is nothing about the structure of the herring that suggests struggling), no great excitement, there was a peculiar jerking of the tail as they performed their revolutions. Soon the object of this peculiar movement was sufficiently evident. Three or four times during each revolution each fish expelled a small white ribbon of milt, which varied from half an inch to three-quarters of an inch in length and was nearly a line in breadth across the centre, but pointed at both ends, and somewhat thinner than it was broad. The delicate ribbons slowly fell through the water, sometimes reaching the bottom almost undiminished in size, but in most instances they had almost completely dispersed before reaching the bottom. In this way the whole of the water about the female became of a very faint milky colour, and practically every drop of it was charged with sperms, as was afterwards ascertained. It will thus be seen that there is no attempt whatever on the part of the males to fertilise the eggs as they escape from the female. While the female is depositing eggs at the bottom the males concern themselves with fertilising the water in the neighbourhood, and it will be observed that the males are careful to guard against the influence of currents. By forming circles around the female, and shedding milt on the way, it matters not how the currents are running, they are sure to carry some of the milt towards the eggs—the milt, like the eggs, sinking though not adhering to the bottom.

This, then, is the natural process of depositing and fertilising the ova of the herring in comparatively still water. I watched the whole process repeatedly, and the details were always the same. When the female had deposited a certain number of eggs at any given spot, she moved forward in a somewhat jerky fashion without rising from the bottom, and as she changed her position the males changed theirs, so that the female was always surrounded by a fine rain of short sperm ribbons. A specimen of hydrallmannia, sent from Eyemouth, seems to indicate that the female moves about amongst

sea-firs and sea-weeds in exactly the same way as she does among stones. On each stem of the colony there is a cluster of ova about the size of a small grape, and all the clusters had reached, on arrival, the same stage of development as if they had been deposited about the same time and by the same fish. It is easily understood how such clusters may be formed if the female is almost in contact with the stems, and there is nothing easier than to form such clusters artificially; the first eggs adhere to the stem, then the others adhere to the eggs already deposited, but being heavy, some of them roll over to the under aspect so as to form the lower half of the sphere, and the result is that partly, it may be owing to the stems moving slightly, there are as many eggs on their under as on their upper surface. It would, however, be extremely difficult to understand how such clusters or how conical masses could be formed on stones if the eggs fell several fathoms before reaching the bottom.

This method of depositing and fertilising the eggs accounts, I think, for all the eggs, or, at least, for a very large percentage of those found attached to sea-firs, sea-weeds, and stones, containing developing embryos.

III. *Experiments made as to the Deposit and Fertilisation of the Eggs.*

When a female was depositing her eggs she was very easily disturbed; whenever anything was introduced into the tank she at once darted off. When strong currents were made she at first seemed to apply herself nearer to the bottom to make sure, as it were, that the spawn would get fixed before it could be carried away; but when the currents were further intensified, she at once changed her position and arrested the escape of the spawn.

A spawning female was held immediately under the surface of the water so as to cause the spawn to escape. When this was done it escaped in long ribbons consisting of a single row of eggs. So firmly do the eggs adhere to one another, that in perfectly still water the ribbon was sometimes over a foot in length before it broke. When it had only about 2 feet of water to travel through it fell in wide loops at the bottom, but when it had over 3 feet to fall the chain broke up into numerous segments which formed an irregular pattern on the bottom. From experiments made it seems the further the eggs have to fall, and the longer they are in contact with the water before they reach the bottom, they are more widely dispersed, and have all the less adhesive power.* When the eggs are expressed in water moving rapidly in various directions the chains soon break into short segments, and the individual eggs and the small groups are

* It may be mentioned that herring eggs if kept moist are still slightly adhesive 24 hours after they are shed.

often carried a considerable distance before they reach the bottom. It will be evident that if the eggs are shed in strong currents some fathoms from the bottom, the chances of their being fertilised would be considerably diminished. When the sea is rough the fishermen seldom expect a good "take," they believe, in fact, that during storms the herring leave the spawning ground, and if the eggs are always deposited as I have described we can easily understand that this might well be the case. Sometimes about the middle or near the end of the spawning period the whole school disappears in a single night. This generally happens during or immediately after a storm, or some other disturbing cause. It may be accounted for by supposing that the herring being ready to spawn, or having already begun to deposit their eggs, finding the conditions on their usual spawning grounds unfavourable, deserted them for banks at some other part of the coast or at some distance seawards.

A number of flat stones and pieces of sea-weed were obtained, and a spawning female held over them at different distances in still water, in water with gentle currents, and in water with strong currents. In this way groups of eggs were obtained which mimicked in a very striking manner all the arrangements of the eggs on the stones and sea-weeds dredged on the Ballantrae Bank. When gently pressed a beaded ribbon, consisting of a single row of eggs, always escaped. When there were no gentle currents it formed conical heaps; when in a gentle current the ribbon fell in irregular loops, the elements of which arranged themselves so as to form a flattened cone; but when strong currents acted on it, the ribbon was broken into fragments, and only a few eggs succeeded in fixing themselves. When the currents were strong, the males were seen not only to swim nearer the bottom, but to expel longer ribbons of milt which reached the bottom before getting dispersed, and remained visible sometimes for ten minutes. On gently expressing a male under the water, it was never possible to expel so fine or so short portions of milt as escaped naturally, but it was extremely easy expelling a ribbon from 18 inches to 3 feet in length, measuring two lines across, and one line in thickness. Such ribbons fell to the bottom, and remained almost unchanged for nearly two hours; they then assumed a segmented appearance, and in about 3½ hours had all but disappeared.

Eggs were allowed to escape into a vessel containing fine sand, and into another containing mud. The eggs after being fertilised underwent the early stages of development, but, either owing to their moving freely about with the sand particles, or owing to their getting coated over with the sand and mud, their further development was arrested. I have not yet determined finally if the development is arrested when the eggs are detached, but this seems extremely probable.

Amongst questions still to be settled are the following:—

How long does the female require to shed all her ova, and does it escape in thick ribbons when she is in an exhausted condition? When the sphincters are relaxed in a dead fish the spawn escapes in rounded cords about three lines in diameter.

How long after death are the eggs capable of being fertilised? Eggs were successfully fertilised during the autumn from a herring taken from the stomach of a cod immediately after it was caught.

How long does the male require to discharge its milt? And how long do the sperms retain their fertilising power? I have found that in $1\frac{1}{2}$ hours the sperms cease moving, and that ribbons of milt from herring dead 12 hours remain for several days unchanged, the dead sperms being unable to separate from each other, and diffuse themselves through the water.

When at Ballantrae I noticed that the trammel-nets secured often more males than females. Is this partly owing to the males swimming somewhat higher than the females, and partly owing to the males taking longer to shed their milt, and hence remaining longer on the spawning-ground? It may be found that while the females discharge all their spawn in three or four days the males require nearly double that time to get rid of their milt. Mr. Wilson, fishery officer at Girvan, at my request, made a number of experiments with ripe herring. He found that on opening a female herring after as much spawn as possible had been expressed by the hand about a fourth of the roe remained, while on pressing a ripe male in the same way about a third of the milt remained, and he observed that it was more difficult to express the milt than the roe. Mr. Wilson states in answer to other queries: (1) That the ripest fish are caught in the trammel-nets, while most of the unripe fish are obtained in the drift-nets; (2) That at the end of the fishing season at least there are about three males taken for every two females, indicating not necessarily that the males are more abundant than the females, but rather that the males remain longer on the spawning-grounds. Boeck states more females are caught at the beginning of the fishing season, which agrees with our observations in the Moray Firth. Mr. Wilson believes that herring prefer quiet water free from strong currents when spawning, and that when the weather is fine, the herring remain long upon the bank, and deposit their spawn leisurely, but when there are strong currents they either hurry the spawning process, or disappear into deeper water.

IV. *The Artificial Fertilisation and hatching of Herring Ova.*

Hitherto, herring ova when wanted for artificial cultivation have usually been obtained in the following way. The herring was removed from the water and gently pressed along the abdomen so as

to expel the eggs by the genital aperture. The eggs as they escaped were received on a plate of glass, and then spread over the surface of the glass by means of a feather. Milt was then added directly from the male, and spread in the same way. After immersing the glass plate several times in water it was introduced into the hatching or carrying box. By this method the eggs were often distributed in a very irregular manner over the surface of the glass, and they were liable to be over-milted. The eggs which first escaped collected in a mass around the genital aperture, and were apt, unless the herring was retained in a perfectly natural position, to run along the side and get contaminated with loose scales, &c., and when they fell on the surface of the glass, or were conveyed to it by the finger or the end of a quill, they formed irregular lumps into which the sperms could not easily penetrate.

I found, after many experiments at Ballantrae, that the best results were obtained when both the male and female were held under water while the milt and roe escaped, *i.e.*, when the natural process of spawning (although not known at the time the experiments were made) is followed.

An ordinary wooden tub was obtained and filled with sea-water. Into this a small quantity of milt was expressed, the male being held completely under water while the milt escaped. A glass plate was then held about 4 inches beneath the surface of the water and the female herring about 1 inch beneath the surface, and then under gentle pressure the eggs readily escaped in the characteristic narrow beaded ribbon, and by moving the fish over the surface of the glass either a close or an open network could be formed. At first, where one loop crossed another, the eggs were two or more layers thick, but either owing to the weight of the eggs or the gentle currents set up in the water, before a few minutes had elapsed the eggs usually formed a single and almost continuous layer, the network arrangement having disappeared. The plate was then allowed to rest for two or three minutes at the bottom of the tub and a few short ribbons of milt were again introduced. After moving the plate once or twice across the top of the tub in order to wash off any scales that were adhering, it was placed either in a hatching or in a carrying box.

Many thousands of ova treated in this way on the 8th March contain extremely active embryos, which are expected to hatch on the 22nd or 23rd of March.

April 3, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The President, on the part of the Council, submitted to the meeting the following address of condolence to be presented to the Queen :—

We, Your Majesty's most dutiful and loyal subjects, the President, Council, and Fellows of the Royal Society of London for the Improvement of Natural Knowledge, desire humbly to offer to Your Majesty the sincere expression of our heartfelt grief at the great loss which Your Majesty and Her Royal Highness the Duchess of Albany and the nation at large have sustained in the decease of His Royal Highness the Duke of Albany.

That a career so full of promise, and one which seemed likely to aid in fostering the interests both of literature and science, should have been so suddenly cut short, only serves to deepen our sorrow.

That it may please Divine Providence to support Your Majesty and the Royal Family in this heavy trial is the earnest wish and prayer of Your Majesty's loyal and devoted subjects,

THE PRESIDENT, COUNCIL, AND FELLOWS OF THE ROYAL SOCIETY OF LONDON.

On the motion of General Walker, seconded by Sir James Cockle, it was resolved: That the Fellows do most cordially concur in the address now read from the Chair.

The following Papers were read :—

I. "Remarks on the Atomic Weight of Beryllium." By W. N. HARTLEY, F.R.S.E., &c., Professor of Chemistry, Royal College of Science, Dublin. Communicated by Professor G. G. STOKES, D.C.L., Sec. R.S. Received March 19, 1884.

In a reply to a note by Professor Emerson Reynolds "On the Atomic Weight of Glucinum or Beryllium," presented to the Royal Society by Dr. Frankland on June 7th, 1883, Dr. Humpidge has made some ...ons concerning evidence which I adduced in favour

of the value 9 or 9·2. I did not consider that these remarks called for notice at the time, as they were beside the question immediately under discussion, namely, the experimental determination of the atomic heat of the metal, but from the fact that they have been abstracted for various journals, and that greater prominence has been given to them than was perhaps originally intended by the author, I beg to be allowed to comment upon them, as my opinions have been entirely misrepresented. Dr. Humpidge states in allusion to me: "This chemist concludes from his experiments that glucinum is a dyad metal, and that its homologues are calcium, strontium, and barium, elements with which it has not the slightest analogy." From this sentence it appears probable that Dr. Humpidge was not fully acquainted with the nature of the evidence advanced, which, however, might be excusable, since though the two papers in which it was contained were read at the meetings of the Chemical Society, that "On Homologous Spectra" on March 15th, and that "On the Spectrum of Beryllium" on April 19th, they were not published in the Journal in time for him to have consulted them.*

The statement quoted above is precisely my argument. "The spectrum of beryllium exhibits no marked analogy with the calcium, the magnesium, or the aluminium spectra, all of which are members of well-defined homologous series."

"There is nothing similar to the boron, the silicon, or carbon spectrum, nor to those of scandium, yttrium, or cerium. The spectrum of lithium is the one most allied to that of beryllium in the number, the relative positions, and intensity of the lines. The question, however, whether beryllium is a dyad, and the first member of the series magnesium, zinc, cadmium, is complicated, since it would probably present a spectrum of a different character to the succeeding homologues, in accordance with the following equation, which follows from the periodic law and holds good for the chemical properties of compounds:"—

$$Li : Mg = Be : Al = B : Si.$$

"The relation of the spectrum of lithium to that of magnesium is obscure, that of boron to silicon less so, consequently we might expect that the relation of the beryllium spectrum to that of aluminium would not be well defined."

It is further remarked by Dr. Humpidge: "And it seems strange that Professor Hartley should consider some slight spectroscopic resemblance between glucinum and the metals of the alkaline earths

* The latter paper, which appeared first, was published in the June number of the "Journal of the Chemical Society," which reached me through the post on June 7th, its date of issue was therefore scarcely likely to be earlier than the 5th. The paper "On Homologous Spectra" was delayed until September.

to outbalance all the weighty chemical and physical differences between them."

The nature of the evidence derived from the spectroscope is here quite misrepresented; probably it was not well understood because of its novelty.

I have found that the spectra of magnesium, zinc, and cadmium are the result of three series of harmonic vibrations with similar intervals, the fundamental vibrations of which differ only in pitch. I have made some similar observations with regard to copper, silver, and mercury, aluminium, indium, and thallium, calcium, strontium, and barium, though maps of some of these spectra have not yet been published. I believe that in series of elements such as these, which exhibit gradational differences in properties and in the properties of their compounds, and approximately equal numerical differences in their atomic weights, we are dealing with the same kind of matter in different states of condensation, or in other words, with matter having similarly constituted molecules, the vibrations of which are in the same direction and at similar intervals but with different velocities.

If we attempt to classify beryllium in accordance with the views of Nilson and Pettersson, the elements scandium and yttrium, with atomic weights 44 and 89 respectively, must yield spectra characteristic of the series of which aluminium is the first member, but it is not possible to find a place for beryllium in this group, nor in those to which cerium, lanthanum, and didymium belong; it is, in fact, by a process of exclusion first and selection afterwards that the element falls into the dyad series.

In the position which I assign to beryllium, we can account for it being related, through the properties of its compounds, to magnesium and zinc on the one hand, and to aluminium on the other.

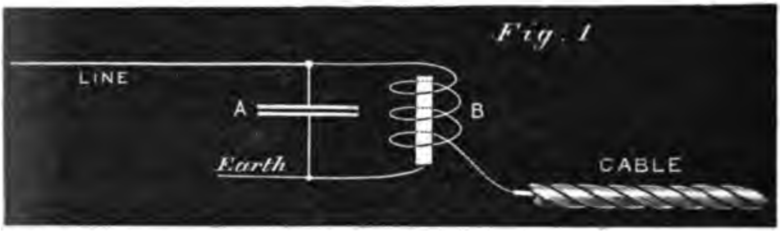
If there is one consideration of greater importance than another which should lead to the determination of the position of an element in a series, it is the mode of vibration of the molecule, and of that we have evidence in the spectrum. When the element is one with a low atomic weight there is no difficulty in interpreting such evidence.

II. "On the Heating Effects of Electric Currents." By William Henry Preece, F.R.S. Received March 19, 1884.

The production of heat in electrical conductors due to the transference of electricity through them has become a matter of very great practical importance. A knowledge of the variation of the law, due to the dimensions and character of the conductor, is essential for lightning protectors and for the leads of electric lights.

Atmospheric electricity has proved a great danger to insulated wires, subterranean and submarine, and to telegraphic apparatus generally. Not only do the direct discharges of atmospheric electricity enter the wires, but very powerful currents are induced in neighbouring wires when these discharges take place, either between cloud and cloud or between cloud and earth. Various plans have been devised to protect apparatus and wires from these currents. Lightning protectors based on the effect of points, on the facility of discharge through vacua, on the low resistance of thin air-spaces to high potential, and on the fusibility of thin wires, have been used.

The most careful and long-continued observations have shown that the survival of the fittest is found, in the use of a thin flat air-space A (fig. 1), supplemented by a fine well insulated protecting wire,



B, of high resistance, wound around a brass rod in connexion with earth. The air-space is obtained by superposing two smooth plane surfaces of thick brass, separated from each other by a space of ·004 of an inch, by means of a frame of mica or paraffined paper. It requires (according to the researches of Messrs. Warren De La Rue and Hugo Müller*) confirmed by my own observations made in Dr. Warren De La Rue's laboratory, a potential of 800 volts to strike across such an air-space, and when this is done a path of no resistance, according to Faraday, is established for the atmospheric discharge to flow to earth.† But destructive currents of induction are often produced in telegraph wires, which have not the requisite potential to strike across this air-space, and the element of *time* may enter to force some of the current into the cable or apparatus so as to

* "Phil. Trans.," Part II, vol. 174.

† Dr. De La Rue has been kind enough to repeat these experiments with the following results:—

Air-space.	Potential.
·001 in.	453 volts.
·002 „	659 „
·003 „	824 „
·00475 „	1030 „
·005 „	1030 „

W. H. P., April 4, 1884.

injure it. Hence a protecting wire has been added, which, while it will allow all working currents to pass without retardation, will not allow a current strong enough to damage the cable to flow through without its being fused. It thus acts as a safety valve. The length and dimensions of this wire have not been hitherto determined by any stringent conditions. After having determined the conditions required to give the maximum effect with the so-called "plate protector"* I became anxious to find out the conditions that would give a similar result with the "wire protector."

Now, the very strongest current that can, under any possible circumstances, enter a wire from the working batteries is one of 500 milliampères (·5 B.A. unit current), while atmospheric currents enter the wires of all strengths from 1 milliampère to 30 or 40 ampères, or even more. Therefore, it was desirable to find that wire which would not fuse with 500 milliampères, but would fuse with currents of about 700 milliampères and over.

Hence, I took a source of electricity, which was a large thermopile of Clamond's make, which would give the necessary current, a rheostat by which it was possible to regulate this current to any strength, a Thomson mirror galvanometer which enabled me to measure, record, and regulate every change of current, and a discharger by which I was able to hold, measure, and adjust the wires to be tested.

1. With a given current and a uniform wire of given diameter and given material, the effect, allowing for the cooling effect of the binding screws, was independent of the length, for whether the length experimented upon was an inch, a foot, or a yard, it always fused when the current reached a certain definite strength. The point of fusion was irregular, for it simply depended on some irregularity in the uniformity of the wire, and the weakest link in the chain went.

2. Samples of platinum wire of various diameters, each of 6 inches in length, were taken, and the current increased gradually until the wire fused. The results are given in the following table:—

Diameter in parts of an inch.	Fusing current in ampères.
·00050	·277
·00075	·356
·001	·437
·002	·790
·003	1·150

* British Association, 1880.

The above figures are the average of numerous experiments, and they are plotted out in fig. 2.

The general law governing the ratio between the current strength and the diameter of the wire when the latter is raised to a definite temperature, and where radiation is free and unchecked by any athermanous envelope, appears to be that the current should vary as the diameter $\times \sqrt{\text{diameter}}$;* this may be proved as follows.

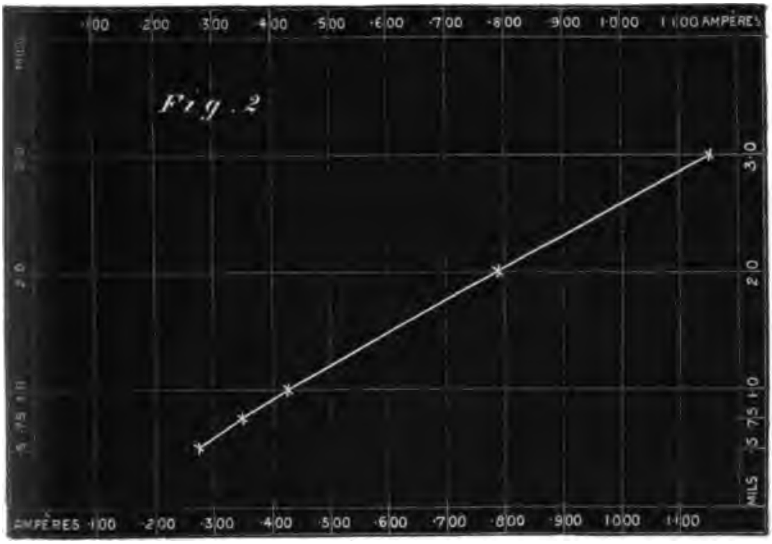


Fig. 2

The heat developed in any cylindrical wire is by Joule's law—

$$H = C^2Rt,$$

where

$$R = \frac{2\alpha l}{\pi d^2}$$

α being the specific resistance, l the length, and d the diameter, then—

$$H = C^2 . \frac{2\alpha l}{\pi d^2} . t = \mu\left(\frac{C}{d}\right)^2;$$

or if we maintain the ratio between the current and the diameter constant, the same amount of energy will be wasted in the conductor.

But the temperature of the conductor is dependent on the rate at which the heat is radiated and conducted away, and when the heat imparted by the current just balances that lost by radiation

* "The Electrical Review," June 24, 1882, p. 454, and Prof. George Forbes, Brit. Assoc., 1882.

and convection, the temperature becomes constant. But the heat lost varies with the surface, and the smaller the surface the less the loss, and therefore the temperature will vary with the diameter. Hence as—

$$H \propto \mu \left(\frac{C}{d}\right)^2,$$

and also

$$H \propto d,$$

therefore

$$\frac{C^2}{d^2} = d, \text{ or, } C = d\sqrt{d}.$$

The results of the foregoing experiments on the current required to fuse wires of different diameters would appear to contradict this law, and to show that the current varies as the diameter, but there are several elements in the method of making the measurements which tend to modify the general law. Platinum wires—especially those of small diameter—are liable to flaws which practically reduce their effective diameter to a great extent; also the larger wires from their greater weight necessarily tend to part asunder at a lower temperature than those which are lighter and on which the strain is less.

3. In thermometric measurements there are only two fiducial points usually employed, viz., the freezing point, and the boiling point, but there are two other points equally well marked, though perhaps not quite so accurately fixed. These are the points of self-luminosity and of fusion. The first point has been determined by Daniell to be $\left\{\begin{array}{l} 980^\circ \text{ F.} \\ 526^\circ{\cdot}6 \text{ C.} \end{array}\right\}$, and by Draper at $\left\{\begin{array}{l} 977^\circ \text{ F.} \\ 525^\circ \text{ C.} \end{array}\right\}$, and it has been shown by the latter observer that this is true for all substances.

I was anxious to discover whether the law before stated would apply in the case of the production of the lowest visible red radiations by currents. I therefore took several wires of different materials, and of different diameters, but all of the same length, and by observing them in a carefully constructed dark chamber, determined the currents that produced self-luminosity, these currents being measured by finding the difference of potential at the ends of a thick German silver wire (R) whose resistance was ·0157ω, inserted in the same circuit. The results of the experiments, which were very carefully made, are given in the following tables:—

Constant deflection from standard Daniell cell = 9050′ (d) through 10,000 ohms.

$$C = \frac{1 \cdot 07}{d \times R} \times D.$$

Description of wire.	Gauge of wire in mils.*	Area per square inch.	Deflection.	Shunt.	Resistance in coils.	Resistance of shunted galvanometer.	Total resistance.	Equivalent deflection through 10,000 ohms (D).	Observed current in ampères.	Current calculated from the formula ·14587 × $d\sqrt{d}$.
Copper.	30	·0007066	285	$\frac{1}{99}$	1,000	512	1,512	4309	23·969	23·969
	24	·0004523	212	$\frac{1}{9}$	10,000	5121	15,121	3026	16·832	17·151
	20	·0003141	163	$\frac{1}{9}$	10,000	5121	15,121	2465	13·711	13·047
	18	·0002544	134	$\frac{1}{9}$	10,000	5121	15,121	2026	11·269	11·140
	14	·0001539	91	$\frac{1}{9}$	10,000	5121	15,121	1376	7·654	7·641
	8	·0000502	113	$\frac{1}{9}$	1,000	5121	6,121	692	3·849	3·300

Constant deflection from standard Daniell cell = 9050′.

Description of wire.	Gauge of wire in mils.	Area per square inch.	Deflection.	Shunt.	Resistance in coils.	Resistance of shunted galvanometer.	Total resistance.	Equivalent deflection through 10,000 ohms.	Observed current in ampères.	Current calculated from the formula ·035755 × $d\sqrt{d}$.
Swedish wrought iron.	60	·002827	330	$\frac{1}{9}$	5000	5,121	10,121	3340	18·579	18·579
	40	·001256	345	0	5000	51,210	56,210	1939	10·785	10·113
	20	·0003141	137	0	5000	51,210	56,210	770	4·283	3·575
	10	·0000785	55	0	5000	51,210	56,210	309	1·718	1·43

* The *mil* is one-thousandth of an inch.

Constant deflection from standard Daniell cell = 9050′.

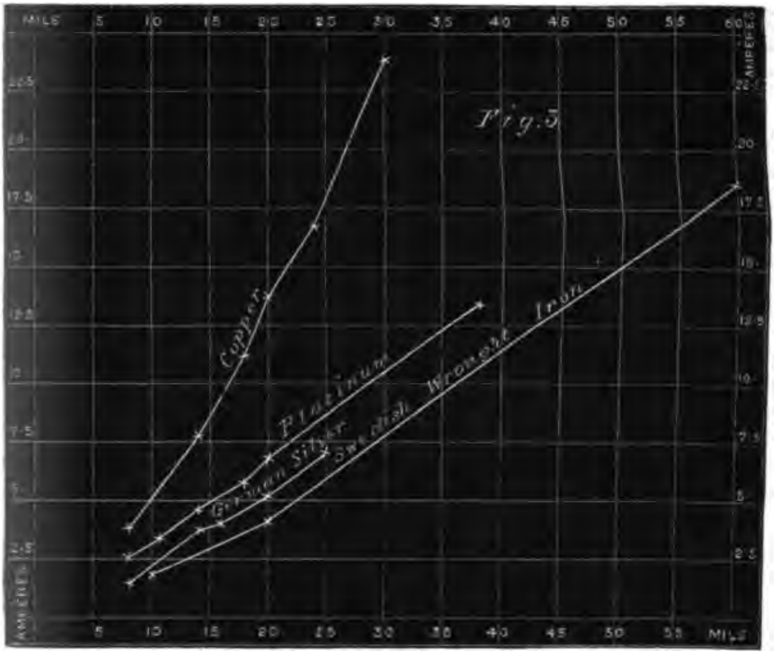
Description of wire.	Gauge of wire in mils.	Area per square inch.	Deflection.	Shunt.	Resistance in coils.	Resistance of shunted galvanometer.	Total resistance.	Equivalent deflection through 10,000 ohms.	Observed current in ampères.	Current calculated from the formula $\cdot 056376 \times d\sqrt{d}$.
German silver.	25	·0004908	207	$\frac{1}{9}$	1,000	5,121	6,121	1267	7·047	7·047
	20	·0003141	150	$\frac{1}{9}$	1,000	5,121	6,121	918	5·106	5·042
	16	·0002010	115	0	10,000	51,210	61,210	704	3·916	3·608
	14	·0001539	110	0	10,000	51,210	61,210	673	3·743	2·953
	8	·0000502	37	0	10,000	51,210	61,210	226	1·257	1·276

Constant deflection from standard Daniell cell = 12,252′.

[illegible] e.	Gauge of wire in mils.	Area per square inch.	Deflection.	Shunt.	Resistance in coils.	Resistance of shunted galvanometer.	Total resistance.	Equivalent deflection through 10,000 ohms.	Observed current in ampères.	Current calculated from the formula $\cdot 057465 \times d\sqrt{d}$.
Platinum.	38	·001134	121	$\frac{1}{99}$	1000 + 488	512	2000	2420	13·461	13·461
	20	·0003141	123	,,	488	512	1000	1230	6·841	5·140
	18	·0002544	104	,,	488	512	1000	1040	5·785	4·389
	14	·0001539	142	,,	88	512	600	852	4·739	3·010
	11	·0000950	100	,,	88	512	600	600	3·337	2·097
	8	·0000502	76	,,	88	512	600	456	2·536	1·300

The results are plotted out in fig. 3. It will be seen that the observed and calculated results agree to a considerable degree of accuracy except in the case of platinum, which behaves as in the previous experiments, and is generally found to be irregular in its quality.

These experiments were made upon wires exposed to the air, where radiation is free. I am anxious to repeat them upon wires covered in insulating material and buried in the ground, but I have not been able to do so up to the present moment. The law with reference to such wires has a very important bearing on the size of electric light



leads, for it shows the necessity of making them large enough to prevent the possibility of their being heated above normal temperatures, otherwise points of danger are very easily reached by increments of current.

III. "Spectroscopic Studies on Gaseous Explosions. No. I." By G. D. Liveing, M.A., F.R.S., and James Dewar, M.A., F.R.S., Professors in the University of Cambridge. Received March 28, 1884.

Having occasion to observe the spectrum of the flash of a mixture of hydrogen and oxygen fired in a Cavendish eudiometer, we were struck by the brightness, not only of the ubiquitous yellow sodium line, but of the blue calcium line and the orange and green bands of lime, as well as of other lines which were not identified. The eudiometer being at first clean and dry, the calcium must be derived either from the glass or from some spray of the water over which the gases with which the eudiometer was filled had been confined. It seemed incredible that the momentary flash should detach and light up lime from the glass, but subsequent observations have pointed to that con-

clusion. Our next experiments were made on the flash of the combining gases inclosed in an iron tube, half an inch in diameter and about 3 feet long, closed at one end with a plate of quartz, held in its place by a screw-cap and made tight by leaden washers. Two narrow brass tubes were brazed into the iron tube at right angles to the axis, one near each end, and one of these was connected with an air-pump, the other with the reservoir of gas. Into one of these brass tubes was cemented a piece of glass tube with a platinum wire fused into it, whereby the electric spark was passed to fire the gas.

The tube was placed so that its axis might be in line with the axis of the collimator of a spectroscope, and the flash observed as it travelled along the tube.

It was seen at once that more lines made their appearance in the iron tube than in the glass vessel, and one conspicuous line in the green was identified in position with the E line of the solar spectrum. Several other lines were identified with lines of iron by comparison with an electric spark between iron electrodes. There could be no doubt that the flash in an iron tube gave several of the spectral lines of iron. We supposed that this must be due to particles of oxide shaken off the iron by the explosion, and proceeded to try the effect of introducing various substances in fine powder, and compounds, such as oxalates, which would give fine powders by their decomposition in the heat of the flame. Several interesting observations were made in this way. When some lithium carbonate was introduced, not only were the red, orange, and blue lines of lithium very brilliant, but the green line hardly less so. After the lithium had once been introduced into the tube, the lithium lines continued to make their appearance even after the tube had been repeatedly washed. When the lithium had been freshly put in, the red line was observed to be much expanded, very much broader than the line given by lithium in a Bunsen burner reflected into the slit for comparison. The light was dazzling unless the slit was very narrow; and it was noticed that if the spark by which the gas was fired was at the distant end of the tube, so that the flame travelled along the tube towards the slit, there was a reversal of the red line; a fine dark line was plainly visible in the middle of the band. When the spark was at the end of the tube next the slit, no reversal was, in general, seen. Later observations showed that some other metallic lines might be reversed in this way, and photographs taken of the reversals. These observations with the eye on the reversal of the red lithium line were made with a diffraction grating, and were repeated many times. They show that there are gradations of temperature in the flame, and that the front of the advancing wave of explosion is somewhat cooler than the following part. The combination of the gases is not so instantaneous that the maximum temperature is reached at once. When some magnesia was

put into the tube the continuous spectrum was very bright, but the iron lines were still brighter. No line which could be identified as due to magnesium was observed with certainty; there was only a doubtful appearance of *b*. With sodium, potassium, and barium carbonates, only the lines usually seen when salts of those metals are introduced into a flame were noticed; but eye observations of this kind are extremely trying, on account of the suddenness of the flash and the shortness of its duration. Thallium gave the usual green line.

Subsequently we had the interior of the tube bored out so as to present a smooth bright surface of iron, and noted the iron lines which were conspicuous in the flash.

For the purpose of identification the pointer in the eye-piece was first placed on one of the strong iron lines given by the electric discharge between iron electrodes, and then, the discharge being stopped but the field sufficiently illuminated, the eye was fixed steadily on the pointer while the gas in the tube was exploded. In this way it was not difficult to see whether any given line was very bright in the flash. The lines thus identified were those having the wave-lengths about 5455, 5446, 5403, 5396, 5371, 5327, 5269 (E), 5167 (b_4). These lines were all many times observed in the way described, and as a rule were always present in the flash. Lines with wave-lengths about 5139 and 4352 were seen, and may possibly have been due to iron, and several more lines were seen occasionally, but were not so regularly seen that they could be well identified. The lines λ 4923 and λ 4919 were specially looked for, but neither of them could be seen. A group of blue lines were noticed, and were afterwards identified by photography, a method much less trying than observations by eye. To give intensity to the photographs ten or twelve flashes were usually taken in succession without any shift of the instrument, so as to accumulate their effects in one photograph. For identification the spark between iron electrodes was also photographed, but with a shutter over the lower part of the slit, so that the image of the spark should occupy only the upper part of the field.

The following is the list of wave-lengths of the iron lines thus photographed :—

4414 ·7	......	4131 ·5	......	3920
4404 ·2	......	4071	......	3902 ·5
4382 ·8	......	4062 ·9	......	3898 ·4
4325 ·2	......	4045	......	3885
4307 ·2	......	4004 ·7	......	3877 ·4
4271 ·3	......	3967	......	3859 ·2
4250 ·5	......	3929 ·7	......	3849 ·7
4201 ·5	......	3927 ·2	......	3840 ·3
4143 ·1	......	3922	......	3833 ·6

3827 ·6	......	3745 ·3	......	N 3580 ·5
3825 ·2	......	3736 ·5	......	3568 ·9
L 3819 ·8	......	3734 ·5	......	3564
3815 ·3	......	M 3727	......	3525 ·7
3799 ·3	......	3719 ·6	......	3496 ·8
3795	......	3709	......	3489 ·8
3787	......	3705 ·5	......	3476
3766 ·6	......	3647	......	3465 ·5
3763 ·4	......	3631	......	O 3440
3757 ·7	......	3618	......	
3749 ·5	......	3608 ·2	......	T 3019 ·8
3747 ·2				

As a rule no iron lines above O make their appearance; in a few plates T is visible, and it is possible that other lines may be obscured by the water spectrum, which always comes out and extends from near *s* to below R. Above T no line at all is visible in any of the photographs, though the spark lines come out strongly enough, and several of the strongest groups of iron lines, both of spark and arc lines, are in the region beyond T.

The spark by which the gas was fired passed in general between a platinum wire and the side of the small brass tube, and was out of view; but in order to make quite sure that the lines were not due at all to the spark, the brass tube was lined with a tube of platinum foil which projected beyond the brass tube a short distance into the larger tube, and the spark passed between the platinum wire and the platinum tube. It was found that the same iron lines made their appearance in the flash whichever way the spark was passed.

Other experiments were made with explosions of carbonic oxide and oxygen, and with coal-gas and oxygen. The explosions of these gases were attended with much more continuous spectrum, and the metallic lines were not always as well developed as they were with hydrogen and oxygen, but on the whole there were as many metallic lines photographed from the flashes of carbonic oxide as from those of hydrogen. There is an uncertainty about the explosion of the carbonic oxide mixture which we cannot account for, even when we take into account the remarkable effects of relative dryness of the gas on the explosions discovered by Mr. Dixon. Sometimes the explosions were so violent as to break the plate closing the end of the tube, though this had resisted the explosions of the hydrogen mixture, while at other times the wave of explosion passed slowly along the tube. The gas was in all cases confined over water and passed directly from the gasholder into the tube.

When the iron tube was lined with copper foil, only one copper line in the visible spectrum, $\lambda = 5104{\cdot}9$, was seen, and in the ultra-

violet two lines, λ 3272 and λ 3245·5. All three lines were very strong, and the two ultra-violet lines were in some cases reversed. These lines were also frequently developed when no copper lining was in the tube, probably from the brass of the small side tubes.

Copper also gave a line in the indigo, λ 4281 about, decidedly less refrangible than the copper line, λ 4275, coincident apparently with the strong edge of one of the bands developed when a copper salt is held in a Bunsen burner.

A lining of copper which had been electro-plated with nickel developed only one nickel line, λ 5476, in the visible part of the spectrum, but gave by photography the following lines in the ultra-violet:—

3807·5	......	3524	......	3445·5
3783	......	3514·7	......	3432
3775	......	3510	......	3422
? 3641	......	3492	......	3413·2
3618·3	......	3461·5	......	3391·5
3612·5	......	3457·7	......	3378·4
3597·3	......	3453	......	3369·6
3571·5	......	3451	......	3367·4
3565				

When nickel oxalate was put into the tube, lines with wave-lengths 3670·5, 3470·3 and 3389·6 in addition to the preceding were developed. It is doubtful whether the line λ 3451 be a nickel line. That at λ=3453 is ascribed to cobalt by Cornu, but it seems to be a nickel line as well.

When copper wire electro-plated with cobalt was put into the tube cobalt lines appeared with the approximate wave-lengths :—

4119	....	3594	....	3492 ?
4089	....	3568	....	3474
3995	....	3528 }	....	3462
3909	....	3525 }	....	3453
3894	....	3523 }	....	3431
3872	....	3502	....	3411
3845	....	3495	....	3404
3601				

The lines λ 3528 to 3522 form a continuous band in the photograph, so that these three lines may not represent the whole group at that spot. It is doubtful whether λ 3492 be a cobalt line as well as a Ni line.

No other metal gave anything like the number of lines that were given by iron, nickel, and cobalt.

A lining of lead gave the lines λ 4058, 3683·3 and 3639·3 strongly, and these lines were frequently developed, though less strongly, when there was no lead lining; the metal being without doubt derived from the leaden washers used to make the ends of the tube air-tight.

A strip of silver gave the lines λ 3381·5 and 3278, and these lines were sometimes reversed. No trace of the channelled spectrum of silver was developed even when silver oxalate was put into the tube, and furnished plenty of silver dust after the first explosion.

A magnesium wire about 2 millims. thick and two-thirds the length of the tube gave the *b* lines very well; that is to say b_1 and b_2 were well developed, and b_4 was also seen, but as the iron and magnesium components of b_4 are very close together, and the iron line had been observed before the introduction of the magnesium, it was not possible to say with certainty whether or not the magnesium line were present too. No other magnesium line could be detected. The blue flame line was carefully looked for, but could not be seen. The photographs showed none of the magnesium triplets in the ultra-violet, nor any trace of the strong line λ 2852, which appears in the flame of burning magnesium, and is yet more conspicuous in the arc when that metal is present.

Metallic manganese, introduced into the tube in coarse powder, gave the group at wave-length about 4029 with much intensity, but no other manganese line with certainty. In the visible part of the spectrum the channellings in the green due to the oxide were visible.

A lining of zinc produced no zinc line, and zinc-dust gave only a very doubtful photographic impression of the line λ 3342. A strip of cadmium gave no line of that metal either in the visible, or in the ultra-violet part of the spectrum.

Tin, aluminium, bismuth, and antimony, also failed to produce a line of any of those substances, and so did mercury which was spread over copper foil made to line the tube.

Thallium spread as amalgam over the copper lining gave the lines λ 3775·6, 3528·3 and 3517·8.

Chromium was introduced as ammonium bichromate, which of course left the oxide after the first explosion. This gave the chromium lines with wave-lengths about 5208, 5205, 5204, 4289, 4274·5, 4253·5, very well and persistently, also the lines with wave-lengths about 3605, 3592·5, 3578·5.

Sodium salts (carbonate, chloride) developed the ultra-violet line λ 3301; and potassium salts give the pair of lines about wave-length 3445; but no more refrangible line of either metal was depicted on the photographs. Lithium carbonate gave, besides the lines in the red, orange, green, and blue, the violet line, λ 4135·5; but no more refrangible line.

Photographs of a flame of mixed coal-gas and oxygen, in which an

iron wire was burnt, show, as might be expected, the same iron lines as are developed in the flash of the detonating gases, and of the same relative intensities. These intensities are not quite the same relatively as they are in the arc spectrum. Thus the lines λ 3859, 3745, 3737, 3735, and 3719 come out in great strength, much stronger than the lines λ 3647, 3631, 3618, which are remarkably strong in the arc.

German-silver wire, burnt in the flame of coal-gas and oxygen, gave the same nickel lines as were given by nickel in the detonating gases, as well as those of copper and lead.

Copper wire gave, besides the lines λ 3272, 3245·5, a set of bands in the blue, which correspond with those given by copper salts in flames, and are probably due to the oxide.

The greater part of the lines which we have observed in the flames of the exploding gases have been observed by us to be reversed when the several metals were introduced into the arc in a crucible of lime or magnesia; which is quite in accord with the supposition that the metals experimented on are volatile, and emit as well as absorb these particular rays, at temperatures lower than that of the arc.

That iron is volatile at a temperature below the fusing point of platinum, which is about 1700° C., has been pointed out by Watts ("Phil. Mag.," vol. xlv, p. 86), who observed in the flame of a Bessemer converter almost all the green and blue lines of iron which we have seen in the exploding gases, besides one or two lines which we have not observed or identified. Having regard to this volatility of iron, it does not seem so surprising that iron lines should be observed accompanying those of hydrogen to great heights in the sun's atmosphere as that they should not be always seen there.

It is interesting to note that Copeland and Lohse ("Copernicus," Dec. 1882) observed in the spectrum of the great Comet of 1882, amongst other lines, four lines, λ 5395, 5369, 5326, 5269, which are nearly identical with iron lines observed in the flash of the detonating gas.

It is remarkable that such volatile metals as mercury, zinc, and cadmium should give no lines in the flame of the exploding gases.

The gases exploded in the tube were generally mixed in nearly the proportions in which they combine chemically; but experiments were made with oxygen in excess and also with hydrogen and carbonic oxide in excess. A small excess of any one of the gases did not seem sensibly to affect the result, but on the whole the metallic lines were more certainly developed when there was not much excess of oxygen, and more constantly developed when hydrogen was used than when carbonic oxide was used.

The absence of any metallic lines in the flame of the exploding gases which are more refrangible than T may be in part due to a falling off in the sensibility of the photographic plates for light of

shorter wave-length; but as the spark lines of iron seem to be quite as strongly depicted on the plates in regions of the spectrum far above T as they are in the regions below, we think that want of sensitiveness in the plates cannot be the only reason for the absence of higher lines, but that the emissive power of the metals for these lines is feeble at the comparatively low temperature of the flame.

This supposition accords with what we have observed of the lines seen in a carbon tube heated by the electric arc ("Proc. Camb. Phil. Soc.," iv, 263). The iron lines photographed as reversed against the hot walls of such a tube were at certain stages of temperature the same, speaking generally, as those we have noticed in the flash of the detonating gases; but as the temperature of the carbon tube rose, more refrangible lines, including all the strong iron lines in the neighbourhood of T, the lines of aluminium near S, and the magnesium line λ 2852, were reversed in the photographs.

Gouy ("Comp. R.," 1877, p. 232), using a modification of Bunsen's burner fed with gas mixed with spray of metallic salts, observed at the point of the inner green flame three or four iron lines which we have not observed in the flame of the detonating gas, the lines b_1 and b_2 of magnesium, two cobalt lines in the blue which we have not seen, one line of zinc, and one of cadmium, and the two strong green rays of silver. Can the appearance of these rays under these circumstances imply that the temperature of the inner green cone of a Bunsen burner, when the proportion of air to coal-gas is near the exploding point, is higher than that of the explosion of hydrogen and oxygen?

The interesting theoretical questions which are suggested by the facts recorded in this paper we must leave for future discussion.

IV. "On the Action of a Secretion obtained from the Medicinal Leech on the Coagulation of the Blood." By JOHN B. HAYCRAFT, M.B., F.R.S. (Edin.), Professor of Physiology in the Mason and Queen's Colleges, Birmingham. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received March 28, 1884.

The following is an offshoot from a more extended investigation upon the coagulation of the blood not yet ready for publication.

The blood flowing from a leech-bite is not readily stopped, often flowing for upwards of an hour after the animal has been removed. The blood within the body of the creature remains fluid for an indefinite time; and when ejected it is found to have lost its coagulability. These are facts known to every surgeon, but they have received no explanation.

While endeavouring to solve one very difficult problem, "why the

blood does not coagulate within the living blood-vessels," the above facts came to my mind and promised to throw some light on the general question.

The explanation which at once suggested itself to me was, that probably the leech secretes some ferment-containing-juice which antagonises the blood ferment, preventing coagulation within its body, enough remaining around the edges of the wound to prevent for some time the outflowing blood from clotting; the blood remaining fluid, in fact, until the leech ferment is all washed away.

It will be seen that this explanation is in the main correct.

In order to investigate its action on the blood, a salt solution extract of leech was obtained. In the first experiment the gullets and buccal cavities of two leeches were removed, cut in small pieces, and placed in 5 cub. centims. of 6 per cent. salt solution. The rest of the alimentary canals were also finely divided and placed in a similar volume of the salt solution. The extracts obtained were of a faint greenish-yellow tint, and alkaline in reaction. A leech was then applied to the nose of a young rabbit for a quarter of an hour, then removed, and held in a salted towel until it had disgorged. Meanwhile the blood flowing from the nose of the rabbit was collected in a test-tube.

The rabbit was then killed, and 3 cub. centims. of its blood received into each of four vessels A, B, C, and D, which were placed under a glass shade, the air within being kept moist with blotting-paper soaked in water.

Vessel A contained nothing but the blood, which coagulated in four minutes.

Vessel B contained in addition 1 cub. centim. of 6 per cent. salt solution, and coagulated in four minutes.

Vessel C contained in addition 1 cub. centim. of 6 per cent. salt solution of the alimentary canals (minus the pharynx) of the leeches. The blood faintly coagulated in four minutes, setting completely in half an hour.

Vessel D contained in addition 1 cub. centim. of 6 per cent. salt solution extract of gullet of leeches. This remained fluid for twenty-four hours, a thick clotted scum forming on the surface when the blood putrefied.

The blood collected from the nose of the rabbit and that from the interior of the leech remained fluid until putrefaction set in, when the same half scum half clot formed on the surface.

It is evident from this experiment that something retarding or preventing coagulation may be extracted with 6 per cent. salt solution from the pharynx or mouth of the leech.

The slight action of the rest of the alimentary canal in retarding coagulation, as this and many subsequent experiments indicate, is due

to some of the secretion diffusing downwards. Its place of origin within the body of the leech is certainly the pharynx or buccal cavity, decoctions of which are more than ten times as strong as those of the rest of the body.

The next experiment was undertaken to test my supposition that this substance belonged to the class of ferments, the most general character of which is that they are destroyed by boiling. At the same time solutions of the leech in distilled water were made in order further to test its solubility.

Equal portions of the blood of a sheep were placed in four vessels containing equal volumes of leech extract.

Vessel A contained salt solution extract of gullet of leech; the blood remained fluid for three hours and was not further observed.

Vessel B contained distilled water extract of gullet; the blood remained fluid for a like period.

Vessel C contained *boiled* salt solution extract; the blood remained fluid for a like period.

Vessel D contained *boiled* distilled water extract; the blood remained fluid for a like period.

The blood received in a test-tube from the body of the animal coagulated in five minutes. This experiment was repeated with similar results.

We have then to do with a substance not a ferment, and soluble in distilled water, as well as in salt solution.

It may be noted that when the leech extracts are added to blood the corpuscles subside to the bottom of the vessel, leaving, in the case of the salt solution extract, a clear plasma, and in the case of the watery extract a plasma coloured by the hæmoglobin, which is partly dissolved.

In order to obtain the active principle of the leech extract in a pure form an attempt was made to isolate it by means of the ordinary solvents.

A watery extract of several leeches was evaporated to dryness, and divided into portions which were respectively extracted for twelve hours with chloroform, ether, benzole, and absolute alcohol. They were then filtered, and the filtrates evaporated to dryness and extracted again with water. They had no action whatever on blood, except in the case of the chloroform extract, and that was very slight indeed.

The residues after extraction with these fluids were dissolved in water, and proved efficacious when added to blood.

The substance is then insoluble in chloroform, ether, benzole, and alcohol.

Its separation in a pure form being evidently a difficult matter owing to its insolubility in the most useful solvents, the attempts at its isolation were in the meanwhile abandoned.

One useful result of this experiment was the preparation of a much purer solution to be used for experimental purposes. The gullets of the leeches were placed in absolute alcohol for a day or two, and then extracted with water. In this way a clear, somewhat coloured alkaline fluid was obtained, almost free from albumen, and giving but a very small residue, this, too, of unimpaired strength. A solution prepared in this way was henceforth always used.

The next task was to find the exact action of this substance on the blood, and in the first case to see if it destroyed or hindered the action of the ferment.

After trying in vain one or two methods the following suggested itself:—

The blood-coagulating ferment is not very soluble in distilled water, so that if a blood-clot containing it be soaked for some time in the watery leech extract, this can afterwards be removed by distilled water without dissolving out the ferment. On adding the latter to hydrocele fluid its activity can be tested (this fluid coagulates on the addition of active blood ferment).

The washed finely divided clot from bullocks' blood was dried and divided into two equal portions by weight. One portion was then placed in a watery solution of leech, and the other in a like volume of distilled water. After twelve hours the clots were removed, dried with blotting-paper, and carefully washed in distilled water. The washing lasted for three hours, the wash water being four times renewed, each portion of clot being treated with the same volume of water. They were then each placed in a few cubic centimetres of 8 per cent. salt solution in which the blood-coagulating ferment is soluble. Five drops of the salt solution extract of the clots were added to equal quantities (5 cub. centims.) of hydrocele fluid. The hydrocele fluid to which the extract of clot, treated with leech extract, had been added did not coagulate at all, while the other portion coagulated in ten minutes.

The action of the leech extract is then seen to destroy the blood-coagulating ferment.

The next question is whether the blood corpuscles are in any way affected by the leech extract? A couple of drops of blood were obtained from the finger-tip, mixed rapidly with a drop of normal salt solution extract of leech, and a minute drop of the mixture placed on a glass slide. This was then covered, and a ring of oil was painted round the edge of the cover-glass to prevent evaporation. When this was examined on a warm stage, the red corpuscles rapidly formed rouleaux in the ordinary way; and the white corpuscles exhibited active amœboid movements which continued for a considerable time—the observations lasted for an hour. In the meanwhile the blood had not coagulated. It is seen then that the vitality of the

white corpuscles is not destroyed, and that the formation of rouleaux by the red disks is not necessarily a phenomenon of coagulation. The coagulation of the perivisceral fluid of the sea urchin is, according to the observations of my friend Mr. Patrick Geddes, not due to the intercellular formation of fibrin in the plasma, but to active amœboid movements of the white corpuscles. The pseudopodia of neighbouring cells join, become welded together, and contract so that all the cells are massed together into a solid clot. Professor Schäfer maintains that there is also a formation of fibrin in the ordinary way which has been overlooked by Mr. Geddes.* I have not examined this fluid myself, and do not feel qualified to speak upon this subject; but certainly the blood of crabs, crayfish, and lobsters clots in the way described by the former of these authors, there being no intercellular formation of fibrin. In this case, then, the leech extract will probably not prevent coagulation. The blood of a crayfish was mixed with one-third of its volume of leech extract. A drop was examined microscopically at the ordinary temperature. The cells exhibited normal amœboid movements, and coagulation was apparently quite normal.

Heretofore we have spoken of the extract of leech gullet and buccal cavity, but it has not yet been shown which is the source of this excretion. All that can be said is that it comes from either the gullet or mouth and sucker of the animal. Careful microscopical preparations were made of the anterior half of the leech, to seek for glandular structures. The animals were hardened for ten hours in saturated picric acid solution, and longitudinal mesial sections were made with a freezing microtome. The sections were stained with picrocarmine. No signs of ordinary glandular structures were to be observed either opening into the sucker, or into the alimentary canal. Some of the epithelial cells forming the integument of the leech are much elongated, passing down even among the subjacent muscular fibres. These have been looked upon as unicellular glands (Ray-Lankester), and in the sucker one may see small collections of these. If the skin lining the sucker be removed, it is found to be active in preventing coagulation. Not in a less degree however is the anterior part of a leech from which the skin of the sucker has been removed. Probably then the secretion is derived from the epithelial cells lining the sucker and buccal cavity; it may be that the unicellular glands of the sucker share in its production.

The products of the digestion of albumin by pepsin have an important action in retarding coagulation. Dr. Schmidt-Mulheim,† working under the direction of Professor Ludwig, has shown that blood received from an open vein into a solution of peptone does not

* "Proc. Roy. Soc.," vol. 34, p. 370.

† "Archiv. für Phys.:" Du Bois-Reymond, 1880, p. 33.

coagulate with its normal rapidity, being retarded for nine or ten minutes. If the peptone solution be injected into the veins of a dog, and blood withdrawn from the animal at intervals after the injection, it will, if the injection be powerful enough (·3 grm. peptone to every 1000 grms. body weight of animal), materially affect its coagulability.

Dr. Fano* finds that certain products of tryptic digestion also retard coagulation in dogs; but curiously enough neither digestion products have any action on rabbits. It may be mentioned that when injected into dogs in larger quantities the blood pressure falls, there being great determination of blood to the abdomen and visceral veins; convulsions are observed, and the animal dies in a state of coma, the blood found within the body being of course fluid.

The coagulability of the blood produced by certain poisons is altogether a question of great interest; for instance, most snake bites (the cobra is an exception) produce permanent fluidity of the blood, and we know nothing either of the relation of that change to concomitant symptoms, or the specific action of the venom on the blood itself.

Now inasmuch as the extract of leech is much more powerful than the peptone solution in preventing coagulation outside the body, its effects when injected into the system may naturally be expected to lead to striking results.

The difficulties in obtaining a license to perform the experiments in England being very great, I availed myself of an invitation from my friend Professor Schmiedeberg, of Strasburg, who kindly placed his laboratory at my service. It is with the greatest pleasure that I remember his kindness and courtesy, I wanted nothing that his well-organised laboratory contained, and to him I owe much personal assistance in an attempt to isolate the active principle present in the leech's pharynx. This attempt, although not as yet successful on account of the great difficulty of the task, will I hope at some future time be renewed with more fruitful result.

In the following experiments the extract of leech was prepared by placing the anterior part of the animal in absolute alcohol for three days, then evaporating off the alcohol, grinding the leech with broken glass, extracting with water and filtering.

Experiment with a Dog weighing 5000 *grms.*—Blood withdrawn by means of a cannula placed in the carotid artery coagulated in 3 minutes.

Temperature 39°·3 (rectum).

5.22 P.M. 20 cub. centims. solution of 8 leeches injected into left jugular vein.

5.25 P.M. 1 cub. centim. of blood withdrawn. It remained fluid until the next morning, never completely coagulating.

* "Archiv. für Phys.," 1881, p. 276.

5.45 P.M. 1 cub. centim. of blood withdrawn, which remained fluid until morning.

6.5 P.M. 1 cub. centim. of blood withdrawn, which remained partly fluid till morning.

6.25 P.M. 1 cub. centim. of blood withdrawn, which coagulated in 25 minutes.

7.0 P.M. 1 cub. centim. of blood withdrawn, coagulated in 5 minutes.

During this experiment the respirations were somewhat increased in number, and the temperature rose gradually until it reached 40°·1 at 7 o'clock.

In the case of peptone injection a marked difference between its effects on dogs and on rabbits is seen. A rabbit was therefore next chosen for the subject of an experiment. A cannula was inserted into the carotid for the purpose of withdrawing blood at intervals.

1 cub. centim. of blood withdrawn. Coagulated in 0·8 minute.

4.40 P.M. 5 cub. centims. of solution from 5 leeches injected into left jugular vein.

4.45 P.M. 1 cub. centim. of blood withdrawn from the carotid. It remained fluid for 12 hours.

4.47 P.M. 1 cub. centim. of blood withdrawn from the carotid. A moiety coagulated in 2 hours.

5.20 P.M. 1 cub. centim. of blood withdrawn. In 25 minutes almost completely clotted.

5.40 P.M. 1 cub. centim. of blood withdrawn. In 10 minutes it clotted fast.

6.0 P.M. 1 cub. centim. of blood withdrawn. In 3 minutes it clotted fast.

In my first experiments when an animal was allowed to live after the operation, the wound in the neck was stitched up in the ordinary way. When examined next morning a large swelling was always observed in the region of the wound. On observing these they were found to consist of imperfectly clotted blood which had escaped from the divided capillaries, the fluid unable to clot had continued to collect often in very large amount, passing down under the skin in front of the chest, and often causing death. Subsequently the wound was carefully painted with perchloride of iron before sewing it up, and no after-bleeding occurred. These facts are not without interest, as they throw light upon a similar symptom seen in many cases of hæmophilia or hæmorrhagic diathesis, where the slightest wound gives rise often to very troublesome bleeding.

It was found by experiment that the injection of the leech extract has no immediate action on blood pressure. After the injection of the substance the animals as a rule appeared somewhat dull, the respira-

tions were somewhat increased and the temperature elevated, but they soon recovered even after injection of large quantities; in one case a rabbit weighing only 1080 grms. was hardly disturbed by an injection of an extract of twelve leeches.

The action of the leech extract, like that of the digestion products of Dr. Schmidt-Mulheim, is not permanent. In a few hours the blood is again normal. This may be from various possible reasons, either that more and more blood ferment is excreted, overpowering as it were the action of the leech extract, or that the latter is oxidised and broken up in the tissues, or finally that it is excreted (probably with the urine). This latter surmise was from the first held by my accomplished friend, Dr. Kobert, and the following experiments show it to be true.

The urine was withdrawn from the dog used in the last experiment 1½ hours after the injection of the leech extract. At the same time urine was obtained from a healthy animal. Two drops of each fluid were added to two equal portions (7 drops) of frog's blood. Whereas the one mixed with healthy urine coagulated in 10 minutes, the other remained fluid for more than 12 hours. The experiment was repeated with a like result.

Before leaving the action of the leech extract on the blood, it may be well to contrast its action with the peptone injection of Schmidt-Mulheim and Fano. In the first place, I should be inclined to doubt whether the active substance in these investigations is peptone at all. It was found that after peptone had ceased to give any reaction in the blood, it was still uncoagulable; the conclusion of Dr. Fano being that it forms some compound with other blood constituents which prevents the coagulation. Is it not more natural to suppose that peptone has nothing to do with the question at all, especially as there are many other digestion products difficult to separate from the peptone. Suppose the same substance formed in the leech were present—which it is not—in the digestion products, it would not have been separated from the peptone prepared by these observers. At any rate, the action of the injection, whatever it is, shows a marked difference in its results from the leech extract. The latter produces far milder constitutional symptoms, is far more powerful in its action on the blood, and has the same action on rabbits as on dogs.*

Since my return to my own laboratory I have investigated the

* My friend Dr. Wooldridge believes that in the coagulation of the blood one factor, at any rate, is the conversion of the white cells into fibrin by the dead plasma ("Proc. Roy. Soc.," vol. 31, p. 413). Without discussing the probability of such a view being correct or not, it certainly is not true in the clotting of the hydrocele fluid by an extract of clot (ferment). In this case my experiments show a special action of the leech extract on the ferment, which, when active, converts large portions of the plasma into fibrin.

action of the leech extract on the coagulation of milk by rennet and on myosin. Rennet mixed with a watery solution of leech has no less power of clotting milk than another portion mixed with water alone. The leech extract does not hinder in any way the clotting of myosin. The latter was prepared from frog's muscle in a somewhat shorter and, I think, a better way than that described by Kühne. Two mortars containing pounded glass were placed in a freezing mixture. One held a few drops of normal salt solution, and the other a like quantity of normal salt solution of leech extract. The legs of a frog were washed out with normal salt solution, the muscles cut out, dried on blotting-paper, placed in the mortar, and ground with the glass into a frozen pulp. This was then tied up in a piece of calico, and the myosin expressed and filtered. Its preparation is most easy, and can be done without fail in the hottest weather. The myosin obtained from the muscle to which the leech extract had been added coagulated in 10—15 minutes, and the other in from 20—25 minutes. The experiment was repeated with like results. It may be remarked that if the clotting of myosin is due to a ferment, this has not yet been isolated.

Inasmuch as during contraction chemical changes occur very similar to those seen in rigor mortis, and inasmuch as in the latter the coagulation of myosin is a prominent fact, some have held that a contraction of a muscle is due also to a partial clotting of the myosin. This view I have never held to be anything but fanciful, for reasons which cannot be stated here. Dr. Lauder Brunton, however, suggested to me, that by soaking rog's muscle in the leech extract, and studying its contractility, some light might be thrown on the subject. I accordingly cut out from the limbs of a pithed frog some of the principal muscles, *i.e.*, the sartorii, carefully removing them with as little injury as possible. Those of one side were placed in normal salt solution, and those of the other in normal salt solution of leech. Comparisons were of course only made between the corresponding muscles of both sides. From time to time the irritability was tested by currents from an induction coil. In all cases the muscles placed in leech extract lost their irritability some time before the corresponding ones placed in salt solution. In one experiment with two sartorii, the left placed in leech extract died in 35 minutes, the right in 70 minutes. Two pectoral muscles attached to the bone lived much longer—one longer than I had conceived was possible—that in leech extract for 12 hours, and the other for 51 hours.

In these experiments those placed in leech extract almost from the first contracted more feebly when stimulated. On more than one occasion I obtained partial recovery when a muscle was removed from the leech extract and placed in fresh salt solution. The only conclusion one can draw is, that the leech extract somewhat hastens the

coagulation of myosin removed from muscle, and probably when it causes loss of contractility in a muscle, it is due to the same reason, inducing, in fact, rigor mortis, the essential phenomenon of which is clotting of the myosin.

In conclusion, then, it may be stated that the leech secretes from its mouth a fluid which destroys the blood ferment without producing any other observable change in the blood. This injected into an animal produces but slight constitutional disturbance, and is eliminated by the kidneys. The action on the rabbit is the same as on the dog; on crustacean blood it is inert. It has no action on the curdling of milk, slightly hastening the clotting of myosin, and hastening rigor mortis.

The Society then adjourned over the Easter Recess to Thursday, April 24th.

April 24, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor Leopold Kronecker, Foreign Member, was admitted into the Society.

The following Papers were read:—

I. "On the Relation between the Electrical Qualities and the Chemical Composition of Glass and Allied Substances." Part I. By THOMAS GRAY, B.Sc., F.R.S.E., and ANDREW GRAY, M.A., F.R.S.E., Assistant to the Professor of Natural Philosophy in the University of Glasgow, and J. J. DOBBIE, M.A., D.Sc. (Edin.), Assistant to the Professor of Chemistry in the University of Glasgow. Communicated by Professor Sir WILLIAM THOMSON, F.R.S. Received March 29, 1884.

The relation between the composition of glass and its electrical qualities has been studied by only a few experimenters, and our knowledge of the subject is still comparatively small. With regard to resistance to electrical conduction through its substance, Dr. Hopkinson has found among other interesting results, that potash or soda-lime glasses have a higher conductivity than flint glasses either light or dense; and his results as to electrical resistance confirm those given below.*

That the presence of a large quantity of alkali in glass is detrimental to its resisting quality has also been pointed out by Ekman.† In two papers ("Phil. Mag.," vol. 10, 1880, and "Proc. Roy. Soc.," vol. 34, p. 199), by one of the authors of the present paper, results are given of experiments on the variation of the resistance of glass of different kinds with temperature, and, more particularly in the second paper, with density and chemical composition.

It was inferred from the results of the electrical measurements

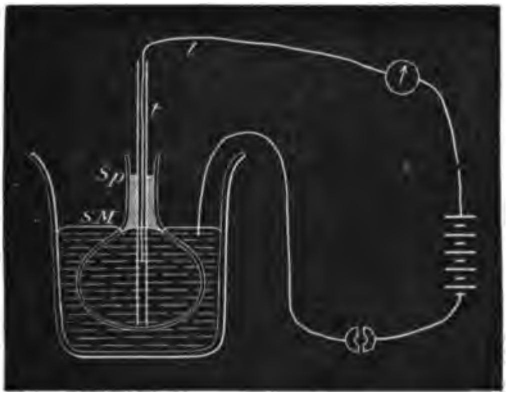
* "Phil. Trans.," vol. 167.

† "Phil. Mag.," vol. 40, 1870.

described in the second paper referred to, and the chemical analyses of the specimens experimented on, that in the case of flint glass, the electrical resistance increased with the density, and on the other hand that for potash or soda-lime glass, the resistance increased as the glass seemed to approach in composition to a definite chemical compound. Density, chemical composition, and resistance appeared all to be so related to one another in the case of the flint glasses experimented on, that increase of density, as well as approach to definiteness of composition, gave increase of resistance.

We offer the present paper as a contribution towards the further elucidation of this question. The experiments which are here described were carried out on glasses which differed considerably from those previously used. They were all flint glasses of considerable density, and it will be seen that though they differ widely in specific resistance, not one can be called a bad glass for electrical purposes. No experiments were made on potash and soda-lime glasses, as these are generally low in resistance, and our first object was to obtain definite information regarding comparatively dense glasses which former experiments show to be relatively much better insulators. It is our intention, however, in a continuation of the present research, to experiment both as to resistance and specific inductive capacity on lime glasses of varying composition.

Fig. 1.



The density of each specimen was carefully determined, and a complete chemical analysis made. The electrical experiments were made in the physical laboratory of the University of Glasgow, and the same general plan was followed as described in the paper mentioned above ("Proc. Roy. Soc.," vol. 34, p. 199). The arrangement of apparatus, with the exception of reversing key, &c.,

is shown in the accompanying drawing, fig. 1, which is taken from the previous paper. The glass which in each case was in the form of a small globular flask of about 7 centims. diameter, and from 2 to 3 millims. thick, was filled up to nearly the bottom of the neck with mercury, and immersed to the same level in mercury contained in an outer vessel. This outer vessel was contained in a sand-bath, which could be heated by means of a Bunsen burner placed below. The flask had in each case a neck several inches long, and one extremity of a wire *l* was passed down the neck so as to dip into the mercury within, while the other was connected through a sensitive galvanometer with a battery of about one hundred Daniell's cells. The other terminal of the battery was connected with the mercury in the outer vessel. To enable the current to be readily reversed, which was done between every successive pair of readings, a reversing key (not shown in the drawing) of somewhat novel construction, and of high insulation, was placed in the galvanometer stand and interposed in the wires joining the battery with the apparatus. It consisted simply of four slender pillars of vulcanite attached to a base piece of the same material. The top of each pillar was thicker than the stem, and had a hollow scooped out of it to receive mercury. In one side of the cup thus made, a piece of copper wire was fixed so as to terminate at one end in the interior of the cup, and at the other to give a projecting piece to which the battery wire could be attached. The hollows were filled with mercury and cross-bridges of copper wire used to connect the cups directly or diagonally, so as to give a current in one direction or the other as required.

The galvanometer used was the instrument described in the "Proc. Roy. Soc." vol. 36, p. 287, to which we refer for a full description, but the following brief account of the instrument may be conveniently given here. It consists of two pairs of coils with hollow cores arranged so that the axes of each pair are parallel and in a vertical plane. The coils act on a needle system of two horseshoe magnets of thin steel wire connected by a very light frame of aluminium, and hung with their planes vertical, so that a horseshoe corresponds to each pair of coils, and has its poles within the hollow cores. Each pair of coils is in this instrument carried by a brass plate, and these plates are set so as to make an angle with one another of about 106°. The needles are not plane, but curved round so as each to lie nearly in a cylindrical surface, the axis of which is the suspension fibre, and which passes through the cores of the coils without touching the coils on either side. Thus the needles can move in the hollow cores through considerable displacements without danger of coming into contact with the coils, and the cores are made smaller than would be otherwise possible. The needles enter the

coils from the same side, and the current is usually sent through the coils, so that one pair cause their horseshoe to move outwards, and the other pair their horseshoe to move inwards, thus turning the needle system round the suspension fibre. This system of needles when rightly adjusted is practically astatic in a magnetic field of uniform intensity. A magnet moved in azimuth by a tangent screw, and vertically by sliding along the vertical supporting rod, is used to give a difference of intensity to the magnetic field at the upper and lower ends of the needles, which are placed with similar poles turned in dissimilar directions.

The instrument is fitted with a distributing plate, the construction of which is fully described in the paper. By means of this plate the coils can be joined in series or in multiple arc, or in any possible combination of these modes of arrangement, so as to give all possible variations of resistance and sensibility, or can be arranged for use as a differential galvanometer when required.

The resistance of the coils when joined in series is slightly over 30,000 ohms, and it can easily be arranged to have a sensibility such as to give, when placed in series with a resistance of 10^{11} ohms in the circuit of a single Daniell's cell, a deflection of one division on a scale placed at a distance of a metre from the mirror. The instrument can easily be made still more sensitive, but when this is done the long period of the needle system renders it difficult to take readings quickly. This disadvantage may, however, be in great measure obviated by using a vane wholly immersed in liquid, or contained in a nearly closed air-vessel of proper size.

With a considerably less degree of sensibility we have found the instrument very convenient for the measurements described below. These were conducted in the following manner. The flask was heated by the sand-bath to a temperature above 100° C., and readings were then taken of the current sent through the glass when the battery was applied by means of the key. That the current measured by the galvanometer might not be affected by leakage through the table and other supports of the apparatus, the wire connected to the inside mercury coating of the flask was carried through air only direct to the galvanometer terminal, and tests were made as to whether the whole current shown by the galvanometer actually passed through the glass by withdrawing the connecting wire from the mercury inside the flask and bringing it into contact with the neck of the flask outside by twisting it round the glass. Such tests always showed that no leakage current existed, and that the current measured was really that passing through the substance of the glass from one mercury coating to the other. After a reading of the galvanometer in one direction had been obtained and recorded, with the temperature of the glass when the reading was taken, the

coatings of the flask were connected together until the next reading was about to be taken. For this reading the current was reversed and the deflection taken with its corresponding temperature, and so on throughout the series of observations in any particular case. The electrification was thus reversed between every pair of readings, and lasted in most cases three minutes. The resistances given below are, therefore, those after three minutes' electrification. The constant of the galvanometer for the battery used was determined once or twice in the course of each day's experiments, so that the resistance of the glass might be easily determined in absolute measure from the known thickness and surface of the flask. These were determined by weighing the flask, first empty in air, then with the globular part filled with water, then when immersed in water to a level a little above the former, to allow for the glass wanting on account of the orifice of the neck. The difference between the first and second weighings gave the capacity of the globe, from which its internal diameter on the supposition of sphericity could be calculated. The difference between the second and third weighings gave the volume of the globe from which, as before, the external diameter, and therefore the thickness of the glass, could be deduced. The resistances were then calculated for each specimen and multiplied by the mean surface in square centimetres and divided by the thickness of the flask in centimetres, so as to give the resistance between two opposite sides of a centimetre cube of the glass in question.

With regard to the results obtained at different temperatures, we have only to say that they confirm the conclusion formerly arrived at, that the conductivity of glass is doubled for every 8·5 or 9 degrees Centigrade rise of temperature. As, however, the object of the present experiments was mainly to further investigate the relation between resistance and chemical composition, they were not made through any very great range of temperatures.

In the following table we give the resistance, density, and chemical composition of each of the specimens experimented on and analysed. We give also results for one or two specimens, the electrical resistance and density of which were determined, but which were not analysed.

The general conclusion from a comparison of these data is, that the specific resistance of the glasses increases with the percentage of oxide of lead, and also with the density. With the exception of the glass marked XX, all the specimens which have been analysed follow the former law, and, with the exception of X and XX, also the latter law, as II, I, and III have practically the same density. But the two flasks X and XX seem anomalous in other respects. They contain for one thing a large quantity of lime as compared with the others, and this, no doubt, tends to diminish their resistance. The density of X

Mark on specimen.	Density.	Specific resistance in ohms.	Chemical composition.								Notes.
			Silica.	Lead oxide.	Lime.	Magnesia.	Oxide of iron, alumina, and manganese.	Potash.	Soda.	Total percentage.	
II	3·141	8400×10^{10}	47·544	40·557	0·250	0·144	1·750	7·598	2·157	100·0	Contains trace of manganese. Silica by diff.
IV	3·144	7250 "									Contains 1·05 per cent. of Fe_2O_3 equal to 0·945FeO.
VII*	3·239	6960 "	..	33·85							
I	3·145	4700 "	55·700	37·098	0·336	0·014	2·500	3·080	1·420	100·148	Trace of manganese.
III	3·141	3868 "	50·104	36·987	0·224	0·198	1·950	8·837	1·700	100·0	Large trace of Mn. Silica by diff. Contains 1·4 per cent. Fe_2O_3 equal to 1·26FeO.
VI	2·854	534 "	62·686	20·606	0·672	0·270	2·00	11·610	2·156	100·0	Silica by diff. Trace of Mn.
V	2·811	453 "	62·263	19·877	0·616	0·216	2·20	11·944	2·884	100·0	Silica by diff. Trace of Mn. Contains 1·5 per cent. Fe_2O_3 equal to 1·035FeO.
X	3·018	545 "	57·50	17·882	2·660	0·360	5·700	8·910	6·838	99·85	Contains 1·2 per cent. of MnO; 3·5 per cent. Al_2O_3; 1 per cent. Fe_2O_3 equal to ·9FeO.
XX	2·829	85 "	58·85	21·423	2·622	0·288	3·357	7·899	5·933	100·372	Contains ·757 per cent. of MnO; 2·1 per cent. Al_2O_3; ·5 of Fe_2O_3 equal to ·45FeO.

* The percentage of lead oxide only was determined for this glass. It was not further analysed.

seems high when the quantity of lead oxide is considered, while the density of XX seems low for the same reason. Also comparing X with XX, we see that while XX contains more lead than X, and at the same time less of impurity, XX has a considerably lower density and a much lower specific resistance. The determinations of density and resistance were repeated for XX, and the results found practically to agree with those previously obtained. It may be interesting to note that, while all the others were new glasses, X and XX were glasses at least ten years old, which had been made for Sir William Thomson's experiments on the preservation of electric charges for a long time in hermetically sealed glass vessels.

The flask marked VII has a somewhat lower percentage of lead oxide than any one of the other glasses which are similar in resisting quality, but it will be noticed that its density is the highest of all. Only a determination of density and of the percentage of lead have been made for this glass. In the case of No. IV, the resistance was determined, but no analysis was made. We ought to state that I, II, III, IV, were all made by Messrs. Osler, Birmingham, and were understood to be from the same pot of glass.

Referring to the table on page 493, and considering the relation of the observed resistances to the amount of soda contained in the glass, we see that, except II and the anomalous glass XX, the resistances decrease as the percentage of soda increases; and making the same comparison for the percentages of potash, we find approximately the same kind of variation of resistance. This seems to indicate that, if other things were equal, the resistances would be diminished by increasing the proportion of alkali, a result which agrees with previous observations. We do not, however, consider the effect of alkali to be so important as the opposite effect of lead oxide.

In the paper referred to above ("Proc. Roy. Soc.," vol. 34, p. 199) it is stated that a glass which seems to approach in composition to a definite chemical compound is also good in point of resistance. It seems possible that in the case of those glasses which have approximately the same composition but different densities, those having the higher densities may approach more nearly to definiteness of composition. At all events, it is well known that minerals, more or less resembling glass in composition, which crystallise, have a greater density than substances of similar composition which are non-crystalline.

It will be seen that the glasses which we have experimented on differ very widely from those analysed by Dr. Divers. Although it is impossible to say in what proportion the silica is distributed among the bases, it may be useful to give the number of combining proportions of the oxides present in the various glasses, and the simplest formulæ which can be deduced from the analyses.

Specimen I.

This glass contains (eliminating traces of lime and magnesia)

$$80SiO_2.15PbO.3K_2O.2Na_2O,$$

or with the qualification which applies in all the cases below, that there is nothing to indicate the manner in which the silica is distributed among the bases,

$$3\{3(PbO.4SiO_2)+(K_2O.4SiO_2)\}+2\{3(PbO.4SiO_2)+(Na_2O.4SiO_2)\}$$

	Found.	Calculated.
SiO_2	57·25	56·6
PbO	38·13	38·57
K_2O	3·16	3·25
Na_2O	1·45	1·43
	99·99	99·85

Specimen II.

$$22SiO_2.5PbO.2K_2O.Na_2O,$$

with elimination of traces of lime and magnesia; or

$$2\{5(PbO.2SiO_2)+3(K_2O.4SiO_2)\}+\{5(PbO.2SiO_2)+3(Na_2O.4SiO_2)\}$$

	Found.	Calculated.
SiO_2	48·56	49·15
PbO	41·44	41·52
K_2O	7·76	7·01
Na_2O	2·25	2·31
	100·01	99·99

Specimen III.

$$61SiO_2.12PbO.7K_2O.2Na_2O;$$

or,

$$7\{2(K_2O.3SiO_2)+3(PbO.3SiO_2)\}+2\{2(Na_2O.3SiO_2)+3(PbO.3SiO_2)\}$$

	Found.	Calculated.
SiO_2	51·32	51·63
PbO	37·88	38·31
K_2O	9·05	8·40
Na_2O	1·74	1·58
	99·99	99·92

It is curious to note that this glass agrees very closely in composi-

tion with a crystal glass analysed by Berthier, to which he assigns the formula—

$$2(K_2O.3SiO_2)+3(PbO.3SiO_2).$$

Specimen V.

$$2PbO.3K_2O.Na_2O.24SiO_2;$$

or,

$$3\{(PbO.2SiO_2)+2(K_2O.5SiO_2)\}+\{PbO.2SiO_2)+2(Na_2O.5SiO_2)\}$$

	Found.	Calculated.
SiO_2	64·21	64·55
PbO	20·50	20·0
K_2O	12·31	12·66
Na_2O	2·97	2·77

Specimen VI.

This flask has obviously the same composition as V.

Specimen X.

Eliminating the lime and trace of magnesia, we have as expressing the composition of this glass—

$$4PbO.5K_2O.5Na_2O.48SiO_2;$$

or, $\{2(PbO.2SiO_2)+5(K_2O.4SiO_2)\}$,

mixed with $\{2(PbO.2SiO_2)+5(Na_2O.4SiO_2)\}$

	Found.	Calculated.
SiO_2	63·09	63·29
PbO	19·62	19·59
K_2O	9·77	10·34
Na_2O	7·5	6·8
	99·98	100·02

Specimen XX.

$$PbO.K_2O\ Na_2O.10SiO_2;$$

or, $\{(PbO.2SiO_2)+2(K_2O.4SiO_2)\}$

mixed with $\{(PbO.2SiO_2)+2(Na_2O.4SiO_2)\}$

	Found.	Calculated.
SiO_2	62·5	61·28
PbO	22·76	22·77
K_2O	8·33	9·62
Na_2O	6·3	6·33
	99·89	100·00

It is curious to notice with regard to XX, which is anomalous in most respects, that it gives a chemical formula widely different from that found for X.

We had hoped to be able to include the results of experiments on the resistances of crystals and minerals. Several sections of different substances have been made, and some of these have been already analysed. Among them are a plate of quartz, cut parallel to the principal axis, and a plate cut parallel to the secondary axes of the crystals, and by means of these specimens we propose to endeavour to find the resistance and specific inductive capacity in different directions relatively to the crystallographic axes. The electrical experiments are not yet, however, completed, but we hope soon to overtake the work.

As the surface of these sections is small, we propose to use the electrometric method by loss of charge, and to eliminate the capacity of the condenser formed by the substance under examination and the electrometer, by using a sliding cylindrical air-condenser, which will allow us to vary the capacity by a known amount.

Our proposed experiments on the specific inductivities of different kinds of glass have been delayed principally for want of a standard air-condenser. We propose, however, now to use an air-condenser, the capacity of which can be altered by a known amount, and Sir William Tomson has very kindly placed at our disposal for this purpose his sliding air-condenser. As soon as Sir William Thomson's standard spherical condenser, which is at present on loan, is returned to the laboratory, we shall be able to verify the results to any necessary extent by comparison.

It was at first our intention to employ the method of electric oscillations used by Schiller* in his experiments on the specific inductive capacity of different substances, but for various reasons we have decided to employ a different method. In the ordinary theory of the oscillatory discharge of a condenser through a coil of large self-induction, it is assumed that the current at any instant has the same strength throughout the whole length of wire in the coil. Now, although this is no doubt approximately true when the coil connects the plates of a condenser of great capacity, it cannot, on account of the considerable electrostatic capacity of the coil itself (a capacity which, taken per unit length of the wire, must vary along the wire in a manner which, so far as we know, has not yet been worked out) be true when the capacity of the discharging condenser is small in comparison with that of the coil.† On this theoretical ground, and also because of the somewhat elaborate and delicate apparatus for measuring very small

* Schiller, "Pogg. Ann.," 1874.

† "On the True and False Discharge of a Coiled Electric Cable," by Professors Thomson and Fleeming Jenkin, "Phil. Mag.," September, 1861; or "Mathematical and Physical Papers," by Sir William Thomson, Art. LXXXIII.

intervals of time which the method renders necessary, we decided, after all our apparatus and arrangements had been planned, to adopt some other method, and we are indebted to Sir William Thomson for the suggestion of a method which promises to be at once easy and likely to give results nearly or altogether independent of "electrification," and we have made arrangements for using it in our continued experiments. One plate of an air-condenser, the capacity of which is known, is joined to the upper end of a fine wire which forms the thread of a pendulum, of which a small metal ball is the bob. Another exactly similar pendulum has, connected to the upper end of its wire, one plate of the experimental condenser, while the other two plates of the condensers are connected to the case of an electrometer used to measure the potential to which the air-condenser is charged. Supposing the air-condenser to be charged, and its potential measured by means of the electrometer, and the experimental condenser to be uncharged, the ball connected with either condenser is drawn aside, and let go so as to fall against the other ball. The balls being of an equal size and of the same material, the first ball will be brought to rest, and the other ball will immediately be set into motion, the time of contact being very short and capable of being approximately estimated. The second ball is caught, and prevented from returning. The charge of the air-condenser has been shared by the contact with the experimental condenser, and the diminution of potential is read off by the electrometer, and from the result the specific inductive capacity of the material forming the experimental condenser can be found; and, as the time of contact is very short, the result will be but very slightly, if at all, influenced by "electrification." We may easily arrange, in ways which we need not here point to, this method for use as a null method.

We propose in a continuation of our experiments to eliminate the uncertainty which still exists as to the effect of impurities in the glass, and to test the effect of varying the proportions of the ingredients, and of adding impurities by making up glasses from pure chemicals. The results of experiments, such as those described above, are so complicated by the presence of small quantities of what may be called "foreign" ingredients, that it seems unlikely that more definite results than those already obtained can be expected as to the exact relation between composition and resistance unless such experiments as we propose be made.

We are also extending our experiments to crystals of quartz, felspar, and other minerals, and to natural glasses, and further to glasses produced by melting natural crystals which are capable of assuming the vitreous condition. We hope to obtain results as to the relation of electrical quality to the crystallographic axes of the crystal.

II. "Influence of Change of Condition from the Liquid to the Solid State on Vapour-Pressure." By WILLIAM RAMSAY, Ph.D., Professor, and SYDNEY YOUNG, D.Sc., Lecturer and Demonstrator of Chemistry in University College, Bristol. Communicated by Professor JAMES THOMSON, LL.D., F.R.S. Received April 15, 1884.

(Abstract.)

The object of this paper is to furnish experimental proof of the theory advanced by Professor James Thomson ("Brit. Assoc. Reports," 1871 and 1872, and "Proc. Roy. Soc.," vol. 22, p. 27), that the pressure exerted by the vapour of a solid substance at a given temperature is less than that of the vapour of the substance in the liquid form at the same temperature.

This relation was specially sought for by Regnault, and, as shown by Professor Thomson, he unwittingly furnished a proof for it in the formulæ devised by him to express the vapour-pressures of ice and of water. Regnault himself, however, from the results of numerous experiments, came to an opposite conclusion, and this conclusion is as yet generally accepted.

A graphic representation of the results obtained by us on heating camphor in a barometer-tube to temperatures ranging from about 150° C. to 200° C. shows (1) considerable irregularity about the melting-point, and (2) that a prolongation of the portion of the curve representing relation of pressure to temperature above the melting-point would intersect the portion below the melting-point.

With benzene, also, the vapour-pressures of which were determined by the method described by us in a paper shortly to appear in the "Transactions" of the Society, the solid-gas curve is evidently not continuous with the liquid-gas curve.

Employing the same apparatus, acetic acid was successfully cooled to temperatures far below its freezing-point without solidification, and numerous extremely concordant observations of the vapour-pressures both of the liquid and of the solid acid were obtained. These observations represented graphically, form two widely divergent curves which intersect in the neighbourhood of the melting-point of the solid acid, 16°·4.

The barometric method was next employed; but the results, like those obtained by Regnault, were capricious. That this capriciousness was not attributable to the presence of air was proved by special experiments, and it remains unaccounted for.

Several very careful determinations of the vapour-pressures of ice and of water below the freezing-point were made. A comparative

method was first employed, in which ice, and water cooled below its freezing-point were simultaneously subjected to the same pressure, which could be varied at will, and the differences of temperature noted.

Another series of observations had reference to the vapour-pressures of ice alone, at temperatures ranging from 0° to −16°. The numbers obtained, when represented graphically, proved not to be identical with those calculated by means of Regnault's formulæ D and E: accepting formula D, which represents the vapour-pressures of steam in contact with water, as correct, owing to the greater number of observations and the greater range of temperature over which they extend, the ice-steam curve was recalculated by the method given by Professor James Thomson, and it was found that our observations agreed much more closely with this curve than with that deduced from Regnault's formula E.

In the original paper, figures and diagrams illustrating these points are given.

As these substances—camphor, benzene, acetic acid, and water—are representatives of very different chemical types, it may be held to be true for all stable substances that the vapour-pressure of the solid is less than that of the liquid at the same temperature, and that the differences between these pressures are calculable from thermic data, where these are known.

In conclusion, attention is drawn to the new method of ascertaining the vapour-pressures of solids and liquids, and a full statement of the important advantages which it offers is given.

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"Observations on the Influence of certain Culture Fluids and Medicinal Reagents in the Growth and Development of the *Bacillus tuberculosis*." By C. THEODORE WILLIAMS, M.A., M.D., F.R.C.P., Physician to the Hospital for Consumption, Brompton. Communicated by Sir JOSEPH FAYRER, K.C.S.I., F.R.S. Received December 28, 1883. Read January 24, 1884.

(Abstract.)

The object of the inquiry is to determine the conditions under which the *Bacillus tuberculosis* of Koch grows and multiplies, and to examine its behaviour under the influence of certain medicinal agents and reputed antiseptics.

The sputum of patients in advanced phthisis was used for experiment, on account of its abundance, the number of tubercle bacilli found in it, and its being easily preserved from decomposition when protected from the air. It was spread on cover-glasses, due precautions being taken to ensure a uniform thickness of the film, and the staining process used was that known as the Weigert-Ehrlich modification of Koch's original method.

Between 200 and 300 specimens were thus prepared and examined with a Zeiss microscope with Abbé condenser, under an F. objective ($=\frac{1}{12}$ immersion lens) and No. 2 ocular, giving a magnifying power of 550 diameters, this being the field generally used for counting the bacilli, and higher powers, up to 1390 diameters, were employed for investigating the structure of the bacilli.

The methods adopted to ascertain the increase or diminution of the bacilli were:—1st, to count the numbers present in a series of fields of the microscope, at least six, and often twelve, being counted, and in doubtful cases the whole slide was carefully gone over before a conclusion was arrived at; 2nd, to note the length of the bacilli and the presence or absence of well-marked divisions in these, preceding their multiplication; 3rd, to observe whether the bacilli are isolated or in groups.

In every case a standard for comparison was first taken from the sputum, and the number of bacilli counted; the rest of the sputum was divided into portions of 20 to 30 minims, mixed with solutions of various medicinal and other agents, and then kept in a Page's incubator at a uniform temperature of 38° C., for periods of from forty-eight hours to eight days. The following cultivation fluids were used:—

Syrup solution of the strength of 2 drachms of syrup to 1 ounce of water.

Hay infusion.

Pasteur's solution (without sugar).
Beef solution, 1 ounce of meat to 2 ounces of water.
" " $\frac{1}{2}$ ounce of meat to 2 ounces of water.
Pork broth (Klein).

Also distilled water and the subjoined medicinal agents in solution were mixed, in generally equal proportions, with the sputum, and kept at the same temperature as above.

Solutions of quinine in strengths varying from 2 grs. to the ounce to 10 grs. to the ounce.
" " arsenious acid, $\frac{1}{2}$ gr. to the ounce and 1 gr. to the ounce.
" " boracic acid, 1 part in 30 and 1 part in 15.
" " iodine, 1 part in 12.
" " perchloride of mercury, 1 gr. to the ounce.

The results were as follows:—

Syrup.—Cultivation was carried on for eight days. The tubercle bacilli were found in diminishing numbers in the first forty-eight hours; they afterwards gradually disappeared, and during the last three days none were detected. On the first day they were of fair length, and some groups were noted.

Hay Infusion.—Four days' cultivation. Bacilli were present in the first twenty-four hours, but afterwards disappeared entirely.

Pasteur's Solution.—Eight days' cultivation. Bacilli diminished in numbers at first and then remained stationary for some days. They showed signs of increase on the eighth day.

Beef Solution.—Two strengths were experimented on during seven and eight days. In the stronger solution the bacilli increased enormously, both in numbers and in length, and in some fields of the microscope it was impossible to count them. Groups of five and ten were not uncommon, but those of greatest length appeared isolated. Some were long and slender, without cross markings, others shorter and slightly broader with well-marked spores. The bacilli were most abundant in the neighbourhood of the meat *débris*, round which they swarmed like ants on sugar. As the solution became decomposed, the number of bacilli decreased, but about the seventh or eighth day, they were seen increasing by division. The *Bacterium termo* and other bacilli were also present. A weaker solution of beef gave the same results, though to a less marked extent.

Pork Broth.—Four days' cultivation. The results were different, for the bacilli did not increase in numbers, but rather declined, though they did not totally disappear from the solution.

Distilled Water.—Two to three days. The sputum of three different patients was mixed with distilled water, and in each case no growth or multiplication of bacilli was observed.

Sulphate of Quinine.—A large number of experiments were made

with this agent, and resulted in demonstrating that the number of bacilli decreases rapidly under its influence, and that the bacilli in the sputum after being mixed with quinine could not be cultivated even in the beef solution before mentioned, thus showing that this drug not only arrests the growth of the bacillus, but destroys its power of multiplication. All the solutions of quinine appear to possess this property, but the stronger ones to the greatest extent.

Arsenious Acid.—This exercised no destructive influence on the bacilli, which increased sixfold in both strong and weak solutions. The rods were generally short, with occasional long ones, and but few spores, and the groups were scanty in number.

Boracic Acid.—The bacilli increased more abundantly in solutions of this than in arsenic, and displayed many groups of 2, 5, 7, and 10. Rods in couples, arranged at an angle of 45°, were common. They were of fair length and many contained spores. Multiplication by division was proceeding.

Iodine.—This drug reduced the numbers very considerably, and in many fields of the microscope it was difficult to find any bacilli, in others a few were present, but these showed no spores or indications of growth.

Perchloride of Mercury caused no diminution, but rather a marked increase in the number of the bacilli. They were longer and with more spores than in the standard solution.

The tubercle bacillus is characterised by great durability of structure, as evidenced by its not being destroyed by the strong acids used in the various processes for its detection, and by its little tendency to decomposition. It does not multiply in distilled water, but does so largely in beef solutions. Arsenic, boracic acid, and perchloride of mercury do not interfere with its development, but rather promote it. Quinine and iodine (especially the former) appear to entirely arrest its growth and destroy its power of multiplication.

OBITUARY NOTICE OF FELLOW DECEASED.

In the death of Mr. CHARLES WATKINS MERRIFIELD, mathematical science generally, but particularly those branches which relate to nautical matters, have suffered great loss. Since the deaths of Rankine and William Froude, no one has passed away whose presence will be so greatly missed at the annual gatherings of the Institution of Naval Architects, and that of Section G of the British Association.

Mr. Merrifield, although essentially a mathematician, and even a pure mathematician, was one of the few to whom the revolution from the rule of thumb to that of exact science in naval architecture was immediately due. The part which he undertook in this movement, although of the greatest importance, is from its nature unlikely to attract notice. And for this reason, perhaps, as well as from the labour involved, as much as its inherent difficulty, was much neglected at the time when Merrifield commenced his work.

It is a common complaint against pure mathematicians, that while they are continually pursuing, or being led by, this subject into abstractions which lie outside the region of experience, they neglect to develop those branches relating to matters of experience sufficiently to render them useful as means of calculation.

Merrifield was an important exception to this rule. He held himself free from the fascination of any line of abstract reasoning which his work may have exposed, and devoted his time and energy to the not less difficult, but to many less interesting task of increasing the usefulness of mathematics by the development and adaptation of methods of application, as well as by the extension of mathematical tables which reduce the amount of calculation (otherwise often prohibitory) immediately necessary for any particular application. To this work he mainly devoted the time at his disposal throughout his scientific career, commencing in 1858; and in 1880, at the request of the Committee of Section A of the British Association, he drew up a report on the entire subject at which he had mainly been working, entitled "Report on the present State of Knowledge of the Application of Quadratures and Interpolation to actual Data." This is published in the British Association Report for 1880, and is in itself a work of great and, in all probability, lasting importance.

But it is not only as chiefly on account of his mathematical work that the loss occasioned by Mr. Merrifield's death will be felt by those who were accustomed to his presence at discussions of scientific matters. His exact and wide knowledge of the circumstances of

applied mechanics, as well as the breadth of his reading and the extent of his knowledge as to what had been done in the way of applying mathematics in almost every branch of mechanics, but particularly in matters connected with hydraulics, together with the interest which he took in, and the ease with which he apprehended, all abstruse matters that might be brought forward, rendered his presence as between an author and his audience almost equivalent to that of a judge in court.

The number of Committees of the British Association on which he sat is some evidence of the number of subjects in which he took deep interest. He held decided though liberal views concerning the best method of education; these he expounded in his address, as President of Section G, at Glasgow, which is printed in the British Association Report, 1876, and is well worthy of the most careful consideration. He strongly urges more language and less grammar, more drawing and less geometry; or, as he himself expresses it, more marching and less drill.

For the following details the author of this memoir is mainly indebted to a notice which appeared in "Nature," from the pen of Mr. J. W. L. Glaisher:—

Charles Watkins Merrifield was born at Brighton in 1827, and died at Hove on January 1st, 1884, in his fifty-seventh year. His education was, as he himself describes it in the address already referred to, mainly classical and legal. He was called to the bar; but had previously accepted an appointment in the Education Department of the Privy Council Office, from which he was promoted to the office of Examiner. His first published paper was "On the Geometry of the Elliptic Equation," in 1858, and was speedily followed by papers on the calculation of elliptic functions, two of which were published in the "Philosophical Transactions." In 1863 he was elected a Fellow of the Royal Society. His mathematical work related to the calculations used in naval architecture, and he became a Member of the Royal Institute of Naval Architects, and contributed a paper to their "Transactions" in 1865.

In 1867 the Royal School of Naval Architecture and Marine Engineering at South Kensington was established, and at the request of the Government Mr. Merrifield accepted temporarily the office of Vice-Principal; and on the unfortunate death of the first Principal, Mr. Purkiss, Mr. Merrifield was appointed Principal. This office he held until, in 1873, the Institution was transferred to Greenwich, when he resumed his office of Examiner in the Education Department. His connexion with the School of Naval Architects added greatly to his previous interest in naval science. He published many papers on this subject in the "Annual" of the school, and in the "Transactions" of the Institute, of which he had become Honorary Secretary.

In 1869 he drew up for the British Association a report on the stability, propulsion, and sea-going qualities of ships; and although he retired from the post of Honorary Secretary of the Institute in 1875, when his services were recognised by a handsome testimonial, he continued regularly to attend the meetings.

He served on several important Royal Commissions, including one on the unseaworthiness of ships, of which the Duke of Edinburgh was President; and during the last few years of his life he frequently sat as scientific assessor to Mr. Rothery in the Wreck Commissioner's Court.

He was at one time President of the London Mathematical Society, of which he was for a long time a member.

The Catalogue of Scientific Papers contains twenty-eight titles under which Mr. Merrifield had published papers between the years 1858 and 1873. He edited many of the text-books of science published by Messrs. Longman, and himself wrote a successful treatise on arithmetic and mensuration in that series. For many years, until prevented by ill health, he conducted unofficially the mathematical part of the May Examinations of the Science and Art Department. He was also, from its foundation, a very active and leading member of the Association for Improvement of Geometrical Teaching.

Mr. Merrifield possessed great literary attainments, being thoroughly well versed in Latin, Greek, French, and Italian. Appended to a translation of his article on Deep Sea Waves, published in the "Rivista Maritima," is a foot-note, which, after bearing testimony to the author's extensive knowledge and excellence of style, expresses the satisfaction of the editor at his adding to those qualifications that of "writing correctly our language."

In April, 1882, when in the midst of his work, he was struck by apoplexy. From this he partially recovered; but he suffered another attack on the 18th October, 1883, which resulted in his death.

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